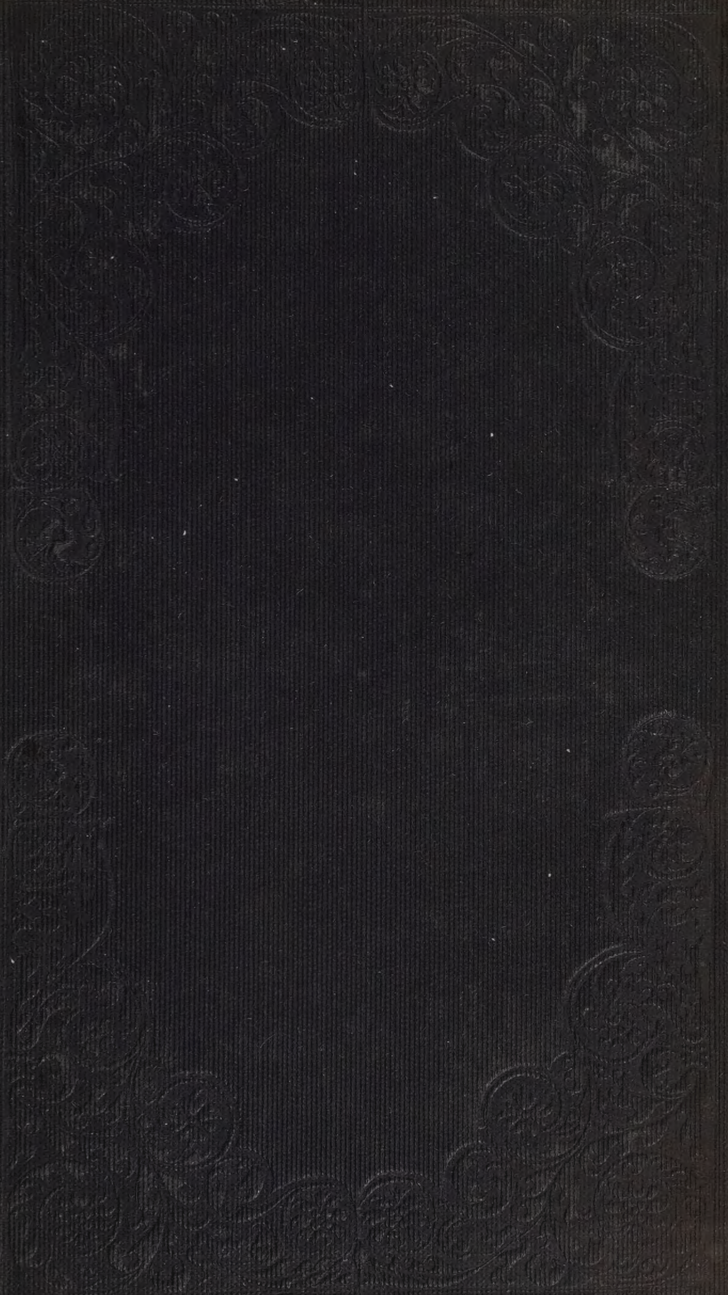

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VOL. XXII. JULY—DECEMBER. 1850.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE,
ETC.

VOL. XXII. JULY—DECEMBER.

1850.

LONDON:
SIMPKIN, MARSHALL, AND CO.
EDINBURGH: OLIVER AND BOYD. DUBLIN: HODGES AND SMITH.
MDCCCLI.

D. I. ROEBUCK, PRINTER, 14, BANK STREET, LEEDS.

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PRACTICAL MEDICINE,

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DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON MORBID POISONS.

By John Simon, Esq., F.R.S., Surgeon to St. Thomas's Hospital, &c.

By *morbid poison* we understand a product, which is the supposed specific cause of certain specific diseases: of syphilis, for instance; of scarlatina, of typhus, of glanders, of small-pox, of hydrophobia, and the like;—a product, which has many striking differences from all other poisons, but chiefly these: first, that while other poisons (oxalic, or hydrocyanic acid, or sulphuretted hydrogen gas) act directly in proportion to their dose, becoming more or less deadly in proportion as more or less of them is brought to bear on the organism; you may observe, contrariwise, that the *morbid* poison (the poison of contagion) produces its characteristic results, when given in the minutest conceivable doses, just as surely, and just as deadlily, as when the system is saturated with it: — and secondly, that while common poisons diminish from the body, or at the most remain stationary, during the production of their effects, morbid poisons apparently undergo, within the body on which they act, a striking and singular increase.

The phenomena which follow infection with a morbid poison consist of certain local changes, attended by a peculiar constitutional state. The *local* changes may be generalised as sub-acute inflammatory processes, attended (perhaps preceded) by the deposition of a *specific material*, which material in most cases contains an agent capable, by inoculation, of producing in another person the same symptoms as have attended its own generation in the original sufferer. The peculiar *constitutional* state is one essentially of depression; modified, no doubt, and intermixed with those phenomena of reaction which the living body (like a spring) always opposes to the direct pressure of exterior influences.

Of the local charges, partaking of an inflammatory character, our memory can give many illustrations: such are seen in the pustules of small-pox; in the cynanche and erythema, and kidney affection of scarlatina; in the intestinal ulcers of typhus; in the catarrh and eruption of measles; in the rupia or periostitis of syphilis; in the swollen parotid of mumps; in the dysentery of malarious fever; in the suppurating tumours of glanders; and in various other symptoms that might be quoted.

And it is because of these local differences in *effect* that we are impelled to distinguish the *causes*, and to speak of them as *specific:* syphilis never produces ulcers in the ileum, scarlatina never causes iritis: the causative poison of the one disease differs from the causative poison of the other, for on the self-same subject it produces different effects.

[But there is probably a *material* in the system, or in the blood, upon which the morbid poison may act.]

What this material—the principle of infective disorders in the human subject—may originally have been, we are totally unable to say, but whatever may have been its first method of generation, we can now confidently speak of it as a possible product of the human body; we know that it is liable to develope itself out of some constituent of the human blood.

What are these constituents? Observation and argument sufficiently show, that the blood corpuscles and albumen can hardly be the constituents in question: first, because, after death by zymotic disease, they are found without evident alteration, and no considerable change in them could escape notice; secondly, because they are indispensable to life, and their even temporary transformation (if complete) would of necessity be fatal; thirdly, because immunity could never be attained by one attack of any particular disease, if it were requisite to exhaust these products: re-exposure to infection would ensure a return of the disease, and a reappearance of its phenomena.

For somewhat similar reasons, we may conclude, that the salts are not the elements concerned. Fibrine and the so-called extractive matters are what remain—can these be the ingredients in question? Substituting for the chemical phrase "extractive matters" the physiological one "waste of the tissues," I am strongly disposed to think an affirmative answer to that question; or, at all events, unhesitatingly to point here as the direction in which accurate pathological investigation may be made with most prospect of success.

For, in the first place, they are matters already in progress of decay, and therefore eminently susceptible of new modification; in the second place, they are in-essential to the nutritive processes, and that removal of them from the system, which would give immunity from re-infection, might be accomplished without withdrawing a vital ingredient from the blood; in the third place, only of such matters as these can it be said, that some of them occur but once in life. In infancy, in early age, and till puberty, there are certain waste materials which never afterwards occur; the temporary cartilages have to waste away, the thymus gland has to decay, peculiar changes referable to the sexual system have to be accomplished, and the effete products of these changes have to be eliminated from the system. And fourthly, notice that the surfaces and organs most prone to affection in the diseases in consideration are those which are eliminative and defecating; those whose normal products can hardly be retained for any time within the body, much less out of it, without undergoing a fœtid decomposition, which sufficiently stamps them with an excrementitious character. Bowels, skin, kidney, tonsils, are the favourite resorts of the several fever poisons, just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated. And it may not be amiss to notice that, whereas the normal and healthy discharge of these substances commonly tends to occur in the highest attainable form of oxidation; and whereas, under a variety of atmospheric circumstances interfering with their efficient oxidation, they must tend to accumulate in forms more susceptible of fœtid decomposition; so it is peculiarly under such circumstances—where ventilation is defective—where human beings are unduly crowded—where the air is loaded with de-oxidizing influences—that zymotic diseases tend to affect the system either through a new generation of their poison, or through some vast increase of susceptibility thus engendered.

On enquiry, it might appear that the relations of infective material to these natural products are definite and constant; that one—let us, for instance, say syphilis—would stand in the particular relation to fibrin; it would be obvious that such an one would be of almost universal inoculativeness, and could only for a very short time, if at all, exhaust the patient's susceptibility to re-infection; and that a drug having certain relations to fibrin (mercury, for example) would interfere with the affinities established by the disease. It might appear that another material, having its origin in the organic waste of nervous substance, would constitute the liability—say, to typhus; such an origin would almost fix the circumstances increasing our proneness to that disease, as well as pre-figure the symptoms attending it. Of another material it might appear that it originates in the infantile decay of temporary cartilage, or of thymus, a decay occurring only once in life; that such material would constitute the susceptibility to measles or hooping-cough, a single attack of which commonly exhausts the patient's susceptibility for ever. Of a fourth material it might appear, that it arises in those changes of blood which attend the inflammatory and reparative processes, under direct atmospheric influence (as in open wounds, cutaneous or mucous), and that in such a product would consist the humoral liability to erysipelatous infection, and to puerperal fever. I need not multiply these hypothetical cases, but, before leaving them, let me beg you to understand, that I employ them only as *illustrations;* that I do not adduce them as pictures of what occurs, only as diagrams explanatory of my meaning.

That the specific materials of the several morbid poisons, as they now pass daily under our notice, constituting the principles of zymotic infection, are either actually derived from the blood, or might have been thus derived, is quite a certainty. Whether each of them, in its first and original derivation, was a native ingredient of the blood, identical with that on which we now see its influence exerted; and whether its first conversion into a specific *materies morbi* occurred without exterior infection, are points which cannot be decided with confidence. In respect of many infected diseases, however, this view of their having first of all arisen spontaneously, would seem consistent with analogy. Experience confirms this theoretical view; for we not unfrequently hear of an outbreak of small-pox or scarlatina, where no communication can be traced with a person previously infected; and we constantly have cases of typhus arising sporadically, where we may fairly consider the patient to have originated the disease within the limits of his own organisation.

It is too much the custom to speak of the personal predisposition to infective disorders, as though it consisted in a condition of mere debility. There is no foundation for this view. If we examine cases of pure debility—such as occur under the influence of extreme inanition, or after severe injuries with loss of blood, or towards the close of chronic exhaustive disorders, we do not find in them any marked liability to the infection of morbid poisons generally. In hospital practice, for instance, we do not find that typhus or erysipelas propagates itself among the patients of a ward in proportion to their weakness. It cannot be too distinctly understood that the predispositions to these various disorders are themselves as various as the disorders, and consist in specific conditions of blood, hitherto very imperfectly explored. Mere debility, as such, has nothing directly to do with the matter: a person is liable to the infection of small-pox, because he has one matter in his blood; to that of measles, because he has another; to that of typhus, because he has a third; to that of scarlatina, because he has a fourth, and so on. A predisposition to one of these disorders is by no means necessarily, and possibly not at all, a predisposition to any other of the class. As regards the exterior circumstances which are considered predisponent to infection, I apprehend there can be little question that their mode of operation must for the most part be indirect: that over-crowding and defective ventilation will increase the liability to small-pox or scarlatina only so far as they hinder a natural elimination from the blood of those materials which constitute the liability to the disease, or as they maintain those materials in a state of imperfect oxidation favourable to the zymotic change. In examining the habitations of the poorest classes in our large cities, we find the atmosphere highly animalised—often fetid with organic matters: the air is so little changed, that it stinks with the volatile excretions of the many human beings crowded together; and to these contaminations are very generally added products of decomposition arising from their other excretions, which lie in cesspools, or have soaked into the soil beneath and around their dwellings. Such an atmosphere can do little towards purifying the blood of matters wherewith itself is already so loaded: the effete matters of the organism, which naturally seek their elimination in an oxidised state, and which for the healthiest elimination ought probably to be in a high degree of oxidation, are here debarred from completing their discharge in its most normal form: and the inhabitants of such localities are consequently maintained artificially, replete with those humoral products which constitute the predisposition to zymotic blood-diseases. It is in these localities, if at all, that such diseases originate *de novo*. I entertain no doubt that some of them do thus originate: though I am unable to state what it is which gives the requisite impetus, or why it is given with one disease oftener than with another. Typhus appears so frequently to arise in this manner, and the predisposition to it is so intimately associated with local circumstances, that some writers have been disposed to overlook the unquestionable evidence that exists of the re-production and multiplication of the poison in the person of the sufferer, and have inclined to consider it an enchorial disease, incommunicable by personal intercourse.

With respect to other alleged predisponent causes, I incline still more strongly to the view already expressed in regard of atmospheric influences, that they can operate only indirectly, only by means of those specific blood-products which constitute the true predisposition in each case. If fatigue predisposes to a particular infection, it can hardly be for any other reason than that the fatigued organ furnishes the material permissive of infection. If errors or insufficiencies of diet, or certain courses of medicine should be found to form a predisposition to certain infectible disorders, their mode of operation could scarcely be otherwise, than by increasing the formation, or diminishing the discharge of that blood product, which is the immediate object of attack to the zymotic poison.

Therefore, as respects those instances of human morbid infection with which we are best acquainted, we may recapitulate our facts, and state our theory, in the following terms: that certain materials of the blood—materials not essential to the performance of its nutritive functions, are, by certain circumstances, rendered liable to undergo definite and specific changes; under the influence of which they become determined, with increased rapidity, to the outlets of the body, and irritate these outlets in their passage; that these changes continue, until the materials affected by them are completely exhausted from the blood; and that the severity and duration of those changes is in proportion to the quantity of material seeking elimination: that the new matters engendered and evolved under these circumstances are capable, in various ways, and with more or less certainty, of producing a precisely similar succession of changes in the blood of another individual, or of any number of individuals: operating always on the same ingredients of the blood as that whence themselves arose, and determining it to the same outlets as that whither themselves were determined; so that the choice of material in the blood, and the choice of outlet in the body, constitute specific characters for the several morbid poisons distinctively, and so that the final products act always as special catalytics for that original material of the blood, wheresoever they may encounter it. But, finally, that under certain possible conditions of accumulation, or tension, in that original material, other circumstances may serve to start it in its progress of specific decomposition, without any demonstrable influence from that exterior catalytic, which is the ordinary occasion of its change.

From our foregone analysis of the pathology of morbid poisons, it is not difficult to deduce philosophical principles of treatment, or to devise a rational explanation of such success and such failure as medicine has hitherto encountered in this department of its ministrations. To check the further conversion of material in the blood; to destroy the poison, or to turn it into harmless combinations; to aid or to anticipate the eliminative efforts of the disease; these would be the indications which pathology would suggest, and these have already, in great part, attained the sanction of experience. But both pathology and practice would concur in adding to these principles another, which, in our present age of palliative medicine, admits of almost infinite application; to remember (namely) that, in each zymotic disease, nature is proceeding in her own way towards a curative termination, and that where (as too commonly happens) we are incompetent to conquer the disease by direct neutralising antidotes, it behoves us chiefly to devote ourselves to the humbler task of moderating local phenomena, and sustaining constitutional power. Thus it is, that, in a vast number of perilous infections, we are able to assist nature through her difficult process of cure, by no other treatment than the judicious administration of natural dietetic tonics—food and wine. Thus it is, that while we recognise the absolute efficacy of mercury against the poison of primary syphilis, we constantly find ourselves without an antidote against its later combinations, and confidently rely on measures adopted, without reference to the specific nature of the disease, solely on the ground of their general invigorating power. Till you can neutralise the poison of typhus, of erysipelas, of scarlatina within the blood of your patients, as you would neutralise an acid or an alkali in a test-tube, never lose sight of this important principle; never forget that these morbid poisons are eminently depressive to life; that they tend to kill by shock and debilitation.

And finally, see what vaccination has done for one of them, perhaps, formerly the most malignant and unsparing. I have taken pains to explain to you the pathology of its preventive power, and very little reflection on the argument of this lecture will convince you that there is, in the nature of things, no reason why small-pox alone should be frustrated in its tendency—no reason why each zymotic disease should not have its own preventive catalytic—no reason why, in connexion with these other pestilences, other men's names should not hereafter be remembered as gratefully and as gloriously as Jenner's in relation to small-pox. Our resources for this great purpose of preventive medicine are not restricted to the teats of cattle. We have the pharmacopœia before us, many of its articles acting catalytically on the blood, and determining products of decomposition, in a characteristic way, to a specific plurality of organs. Not only is there no reason against the possibility that many of these medicinal catalyses may be preventive of the zymotic catalyses; but there is every reason for such a possibility. To give you an illustration, why should not belladonna (determining the products of its operation to the throat, the kidneys, and the skin) act as a medicinal catalytic of that material which constitutes the susceptibility to scarlatina, and thus, in recognised reality (as heretofore in vague tradition) be preventive of that disease? Again, why should not the direct counteractive influence of drugs be extended in respect of these diseases, when they already are in attack? Why should we not be enabled by one drug to arrest the blood-change of typhus, and by another that of plague or glanders, just as with quinine we render the blood insusceptible of further detriment from the malarious poison.—*Lancet, August* 24, 1850, *p.* 227.

2.—ON HAY FEVER AND ITS TREATMENT BY NUX VOMICA.

By G. T. Gream, Esq.

Hay Fever or Hay Asthma is (in England) a well known complaint, by which many—especially females, and those of irritable surface—are annually distressed during the droughts of summer. Daily, or more frequent, paroxysms of difficult respiration, severe prolonged sneezing, and a burning pain in the nostrils, eyes, and face, are the usual symptoms. Dr. Craigie and others doubt the common explanation of the cause of the affection, viz.: that it depends upon irritation of the peripheral extremities of the imperfectly protected nerves of the nostrils, &c., by the subtle pollen of the innumerable flowers, especially the grasses, which bloom in May and June, and with which the atmosphere must, in some localities, be greatly charged. In the vicinity of hay fields and wild pasturage, if the weather be dry, those who are susceptible are certain to suffer; whereas, a rainy season, or sojourn on the sea-coast, will enable such persons to escape. Observation of many such facts has fully satisfied us, that there is no good ground for the incredulity expressed in the following passage by Dr. Craigie. "A particular variety," says he, "of catarrh, most prevalent in the summer months during the inflorescence of the hay crop, in certain situations, has been believed to be connected with some irritative vapour exhaled from the flowers of some of the grasses, and has therefore been distinguished by the name of hay fever. It is doubtful whether this idea of the origin of the disorder be well founded; and it seems quite as likely that it is produced, as other varieties of catarrh, by imprudent exposure during excessive heat. The liberties which are often taken during extreme hot weather, are sufficient to induce catarrhal disorders, without having recourse to the assumption of a peculiar emanation." (*Practice of Physic*, vol. i, p. 824: Edin. 1837.) We will venture to say, that no one who had seen such cases of hay fever as abound at this season, and as we have had to deal with during the present and former seasons, and who has carefully investigated into their personal and topographical peculiarities, could have written the paragraph just quoted. The popular theory is correct, that hay fever bears no relation whatever to common catarrh from exposure to cold. We feel assured that the cause and the cure in the two complaints are totally different.—*London Journal of Medicine, July*, 1850, *p.* 699.

[The above remarks were made on reading the following paper by Mr. Gream on this subject:—]

The efficacy of nux vomica as a remedy for hay fever is not generally known.

The symptoms which are removed by its administration are, distressing paroxysms of sneezing prolonged until a state of depression ensues, and preceded by excessive irritation of the Schneiderian membrane, the conjunctivæ, and the face; the latter being heated at the time of the paroxysm, and during the intervals, as if a hot plate of iron was nearly in contact with it.

I believe that the farina of grass has no more influence in causing the disease than that of any other flowers; in hay-fields and hay-lofts there is necessarily more dust than elsewhere—but the dust from beaten carpets, from the roads, and from other sources, produces the same distressing symptoms. I am led to think that in the middle of summer, from the end of May to the end of July, at which time hay fever generally ceases, a quantity of fine dust floats in the atmosphere, finer than any which is in the air at other seasons, increased probably by the farina of the mass of flowers at that period in bloom, but that during later and earlier months, the more frequent rains, and the dews at night, prevent these particles from leaving the ground; and I have been induced to suppose that this reasoning is correct, by finding that, however distressing the symptoms have been during the day, they are all entirely removed upon the occurrence of a shower of rain; the face becomes cool; the irritation of the nostrils and of the eyes ceases, and does not return until the heated atmosphere has again evaporated the fallen rain.

The relief afforded by a residence near the sea appears to be owing to the absence of fine particles of matter near so large a surface of water.

I am indebted to my friend Mr. Hammerton, of St. George's Hospital, for suggesting to me the nux vomica as a remedy in this complaint, which has frequently caused me, personally, much annoyance; it was administered by a friend of his to large numbers of the country people in his neighbourhood, who flocked to him annually for relief, having experienced so much benefit from it.

Having taken it for three years with decided effect, and having for nearly that time prescribed it for others, with equal success, I feel bound to publish it.

The preparation recommended, and which I have always prescribed, is the tincture of nux vomica of the "Dublin Pharmacopœia." Ten drops of this should be given for a dose, in water, and increased gradually to twenty drops, three times a day; the action of it should at first be watched. It is an agreeable light bitter; increases the appetite; and influences the Schneiderian membrane, no doubt through the medium of the nerves.

I have accompanied the administration of the tincture with the application of an ointment (as high up in the nostrils as possible) composed of one drachm and a half of Goulard's extract; two ounces of spermaceti cerate, and a few drops of oil of roses or of bergamot.—*Lancet, June 8, 1850, p.* 692.

3.—ON CANCER.

By John Simon, Esq., F.R.S.

[In Mr. Simon's admirable Lectures on Pathology, lately published in "The Lancet," we have been particularly interested with the subject of *Cancer*. Speaking of cancerous tumours, Mr. Simon says, that in this term several varieties are included, which, although differing in many particulars, yet in all essential ones agreeing so truly as to establish between them a fundamental similitude, and justifying the pathologist in classing them in the same morbid family. For their differential characters, &c., Mr. Simon refers the reader to Rokitansky's incomparable work, as published by the Sydenham Society, and the admirable monographs of Dr. Walshe and Dr. Bennett. Mr. Simon goes on to say,]

What are the anatomical elements of encephaloid tumours in general? Essentially forms and steps of microscopical cell-growth: round or oval cells, like pus-globules, with dotty contents, and with a nucleus more or less distinct; or cells, like those of glands or ganglia, more opaque with granular material, round or angular, or developed into processes, and having one nucleus or several; or gigantic mother-cells containing within them simple cells of a new formation, or nuclei and granular matter; or cells in various degrees of blackness with pigment, perhaps to an amount which shall render the whole mass *melanotic;* or spindle-shaped bodies with oval nuclei, indicating the commencement of new fibre; or free nuclei—some round and oval, with single nucleoli, having the size of normal gland cytoblasts, others twice or thrice as large, elliptical, with double nucleoli; or elementary granules of all sizes, or glomeruli consisting of them. And, for the intercellular material—it has no high development in such cases: sometimes it will be so amorphous and incoherent, that the microscope cannot identify it; sometimes it will be more fixed, but scarcely more shaped, having the faintest appearance of fibre, and carrying a few elongated nuclei in its substance; sometimes in this form it will be more distinctly membranous, opaque, and fibrillated; sometimes spindle-shaped bodies with nuclei will be arranged as septa; sometimes there will be a developed fibrous frame-work, forming distinct alveoli for the cellular elements of the tumour; sometimes, though very rarely, there will be a bony stroma for the growth, supporting it, not at its base in the form of an osteophyte (as when in contact with normal bone) but actually spread throughout the tumour, as a fine diploe, within which the cancerous germs lie as marrow.

To complete this statement, I should add that the products of hæmorrhage in greater or smaller quantities will often be found in these tumours, constituting the variety of *fungus hæmatodes;* that large granuliferous cells, with more or less abundance of free oil, will be seen in them, often gathered in clusters, which to the naked eye seem yellow, often diffused in lines which run together in plexuses, and constitute an appearance, which Müller believed to be characteristic of one species of cancer (hence named *reticulatum*) but which belongs to all at one period of their development; and finally, cysts will sometimes be noticed in combination with these encephaloid elements, and may or may not have their cavities partially occupied by the further growth of such elements in their interior: the latter possibility being chiefly illustrated in the breast, brain, and testicle.

The chemical constitution of encephaloid cancer is almost as brain-like as its appearance: albumen and fat are its main ingredients: the former in overwhelming proportion, the latter in varying quantity (sometimes very considerable) and in quality closely resembling the cerebral fats of the human subject.

The materials which during life run from the ulcerated surfaces of such a tumour are identical with those which post-mortem examination displays in its interior structure; and hence it is, that in every instance of fungating sore, the microscope can inform us whether the discharge be the product of cancer, or of simple suppuration.

I have now described to you the ordinary characters of encephaloid cancer, sufficiently to furnish a text for such pathological remarks as I have to make on it; and sufficiently also to serve as a standard of comparison for the other two so-called species of cancer, *scirrhus* and *colloid.* I have sought to give chief prominence to the encephaloid form of the disease, because I believe it to be cancer κατ' ἐξοχην; because I believe it to be the highest and completest expression of that morbid tendency which constitutes the cancerous diathesis; because, in a word, I believe it to be of all cancers the most cancerous.

The distinctions of the other species are these:—*Scirrhus* derives the sensible qualities which give it its name from its greater fibrousness of structure; there is spread through its substance a continuous uniform network of filamentous tissue, or a radiation of fibres from several separate dense centres, or a coursing of ligamentous bands, which branch and anastomose; and in the interstices of this rigid plexus, there lie microscopical elements, identical with those of encephaloid; identical (that is to say) with many of the cell-forms which I have enumerated, though not often presenting that excessive development which leads to endogenous cell-growth, or to the formation of large caudate cells; but showing, in preference, an abundance of those shapes which are transitional to filamentous tissue—spindle-shaped bodies, and nucleated fibres, in various stages of their growth. The scirrhous mass compounded of these elements is often very hard and tough, feeling like cartilage, and cutting like ligament; its section shows a bluish-white colour, and seems glossy and semi-transparent where the mass is firmest; but, in points where softening is in progress, the surface appears more opaque and buff, presenting that yellow *reticulum*, or other yellow accumulation, which I have already spoken of, as depending on an oily transformation of its material into the substance of compound granuliferous cells. I may add, that scirrhus is far less vascular than encephaloid cancer; that it grows less rapidly, and remains within much narrower limits of size; and that it is rarely the seat of interstitial hæmorrhage.

Colloid deviates from the standard of encephaloid, usually, in a different direction to scirrhus. It is even less vascular than this disease; but, unlike it, it is often soft, gelatinous, or gummy: and yet this does not depend on the absence of fibrous tissue, for some forms of the disease have an exceedingly distinct filamentous frame-work: the softness of such specimens depending evidently on the peculiar nature and copiousness of that material which lies within their fibrous plexus, and which constitutes the bulk of the morbid mass. This characteristic material is a colourless, transparent jelly; in substance something like the crystalline lens; often as soft as the outer layers of that body, sometimes harder than its inmost layers: where scattered on membranes in the form of small rounded masses, it suggests to the mind the thought of a drop of quite colourless gum, fallen on the surface and dried there in its globular shape. A thin section of this extraordinary product shows in most instances that the gelatinous ingredient lies within very distinct loculi; often so distinct, that from this circumstance the disease has been named *alveolar cancer*. I should hesitate to say that, as a rule, the fibrous tissue is more developed in colloid than encephaloid cancer; for, on the one hand, its greater perceptibility may be due, in a chief degree, to the extreme translucency of the intervening jelly, which sets these fibres off as though they were dried in Canada balsam; and, on the other hand, it is frequent to find specimens of colloid, in which (as in many of encephaloid) no fibrous net-work is perceptible, but in which the only trace or rudiment of matrix will consist in the faintest, almost imperceptible hyaline material, disposed reticularly, with some few elongated nuclei in the axis of its distribution. The intralocular material appears to me more peculiar in its physico-chemical qualities, than in respect of the organic shapes which occur in it. Sometimes a quantity of jelly will seem amorphous under the microscope; sometimes ordinary round or oval uni-nucleated cancer-cells, with fluid transparent contents, will be found; sometimes (though I doubt if this be not a partial admixture of alveolated encephaloid) a loculus will be full of simple cells, tending to become opaque with granular proteinous contents; sometimes a large granular mass, itself opaque, will lie amidst clear blastema, within a loculus, will be bounded by no distinct membrane, will not be resoluble into separate cells, but will contain a number of nuclei diffused in its substance; sometimes (though less often than is supposed) it will contain true mother-cells, having a smaller cell-brood within them; sometimes it will present chiefly naked nuclei and nucleated fibres traversing its jelly at long intervals of distance, and associated probably with an abundance of oily globules; the latter elements may exist separately, or may be clustered in compound cells, or may be ranged in long branching lines which seem to follow the incipient fibrous organization of an interlocular matrix. All these ingredients are substantially the same as those of the other forms of cancer, differing only (so far as the eye can judge) as to the translucency and gelatinousness of the blastema in which they grow, and perhaps also as to the arrangement of their fibrous skeleton. With those other forms of cancer, moreover, colloid is ready to combine itself, and especially with encephaloid; which not only may unite with it in the construction of one tumour, but may arise as its secondary consequence, when colloid, as a primary disease, has contaminated the lymph or the blood.

With respect to the *habitat* of cancer (taking it again generically) the following, according to Rokitansky's large experience, is the order of choice in which the several organs of the body are primarily attacked by it—viz., most frequently the uterus, the female breast, the stomach, the rectum; next, the lymph glands, the liver, the peritoneum, the bones; next, the skin, the brain, the globe of the eye, the testicle, the ovary, the kidney, the tongue, the œsophagus, the salivary glands. As regards the species: colloid prefers the stomach, the rectum, the peritoneum; scirrhus, eminently the female breast; next, the intestinal canal and especially the stomach; encephaloid may occur in all organs, but there are some in which no other form of cancer ever exists—the liver, the kidney, the lung, the testicle, the globe of the eye, the lymph glands; and it constitutes the only form in which a secondary development of the disease is known to occur.

Cancer may exist simultaneously in several organs of the body; but, where this is the case, we are generally able to distinguish, that in one organ its appearance has been *primary*, and that the other organs have suffered in *secondary* or *tertiary* succession. Its first outbreak is commonly in a single organ; and this first focus of the disease has a faculty, which in some respects is peculiar to cancer, of diffusing its contamination through other parts of the system. There are three manners in which this contamination may arise:

["First," says Mr. Simon, "it seems probable that if the germs of cancer alight and are retained even on simple continuous surfaces, they can fructify and lead to the development of a tumour, the disease here *transferring itself by immediate contact*." Secondly—"If by some lesion of the veins they are allowed to enter the circulation, they will become centres of cancerous growth in the substance of whatsoever organ the blood may carry them to and fix them in." Thirdly—"That method of propagation which is effected by the lymphatic canals, and which consists essentially in transference, *by continuity of blastema*."

Proceeding now to the pathological affinities and contrasts of cancer, Mr. Simon continues:—]

Thrust aside all the accidents and trivialities of the subject, strip the question naked, and what does a cancer mean? Substantially it is a new excretory organ. Under the pressure of some mysterious constitutional necessity, a growth arises, which (in its typical form) tends essentially to acts of eliminative secretion; just as distinctly as the healthy liver or the healthy kidney. You must not stop short at the *tumour*: you must scan the whole drama of the disease, in which the tumour forms but a pro-œmium. Look again at such a case as I gave you for illustration—a case of encephaloid manifested in the globe of the eye: that vast fungating ulcer which ensues on the protrusion of the tumour, and which continues hourly to purge forth its profuse discharge —that is the real intention and purpose of the tumour, as palpably as urine expresses the intention of the kidney, or bile the intention of the liver. The cells of the tumour have grown like healthy gland-cells—like the nucleated cells of a mucous membrane, only to discharge themselves with their contents. There is nothing, I repeat, like hypertrophy in the disease: it consists essentially in the establishment of a new vent —a new organ of elimination; and that organ developes itself according to the type of other secreting organs; it developes itself as an apparatus for the formation of deciduous cells.

How constitutional—how intensely constitutional—is this eliminative action, can hardly be misunderstood. The mere fact that nature thus (if I may say so) deliberately, and without any exterior provocation, organizes in one spot of the body a drain which becomes so ample as to starve the remainder; the circumstance that this flux is not a single emptying out of some previous local accumulation (such as one sees in the pointing and discharge of an abscess) but is a persistent process, depending day by day on new acts of growth; the observation that the tendency to the disease is in many instances hereditary; the existence of a marked cachectical state which precedes, as well as accompanies, the evolution of the tumour; and, most of all, that flagrant evidence of metastasis which daily renews itself for our instruction, consisting in the failure of surgical operations, by reason of the obstinate emergence of the disease, even again and again, in parts previously uncontaminated by its presence:—these arguments are to my mind as conclusive for the constitutional and purely constitutional origin of cancer, as any which I could adduce to show you the constitutionality of small-pox or gout.

I infer that the formation of a cancer in the body is not so easy and off-hand a process as the local determination of gout or rheumatism; I infer, either that a special cell-growth is necessary, which it takes time, with some intensity of cachexia, to construct; or at least that the existence of such a cell-growth promotes, in a remarkable degree, the ulterior development of cancerous phenomena; and, in either case, that the system will tolerate a not inconsiderable degree of whatsoever conditions form the cancerous diathesis, without proceeding to construct a cancerous tumour *de novo*, when every atom of its primary structural manifestation has been thoroughly removed from the system. Even in gout, where the facility of local organization is comparatively great, we know how long a time may elapse, and how much general illness may arise from gouty blood, before the disease first succeeds in centralizing its products at the toe, or elsewhere; and if that toe were removed, there would probably be another lengthened period of latency, before a new focus of gouty inflammation could be established. And this difficulty exists, as I have said, to a much greater degree in regard of cancer, owing to its requirement of a special structure for its eliminative acts; whence we may confidently argue, that cancerous diseases have a chronic period of latency, during which their cachexia acquires intensity from accumulation, till at length it suffices to establish the local vent; and we may argue further, that the disease can be again reduced to this state of latency, and to the accompanying difficulties of evolution, if all such local conditions be removed as favoured the first localization of its products. And here lies the practical importance of these considerations; for it is exactly in that chronicity which we have traced in the constitutional disease, and in the reluctance with which Nature proceeds to a total reconstruction of the cancer-growth, that the chance, (such as it is) of doing good by surgical operation, singly and entirely consists. And the whole tact and discrimination of the surgeon, in deciding whether an operation may probably lead to cure of the disease, have to be brought to bear—first, on determining the total abstinence of any secondary deposits, or any irremovable extension of the growth, which might serve as a centre of new development; and secondly, on measuring the intensity of the constitutional cachexia.

In respect of the latter point, there are many obvious considerations, as to the rapidity with which the tumour has been evolved, as to the state of activity in which it actually is, as to the patient's apparent general health, and the like. But I pass over these topics (as better adapted for clinical illustrations) in order to suggest to you the sort of guidance which you may derive from the site of the disease, from its species, and from the amount of local influences apparently concerned in its production.

As respects the pathological relations of the forms or species of cancer, each to the other, it seems to me that the cancer is a perfect expression of the cachexia, in which it arises, in proportion as it contains the means of fulfilling its eliminative destiny. Encephaloid, consisting entirely of cell-growth, and hastening to discharge itself, seems the most characteristic representative of what is fatal in cancer—a view additionally confirmed by its rapidity of evolution, and by the empirical evidence of its habitual return after operation. Of colloid I can scarcely offer an opinion; it is too rare, especially in surgical practice. Scirrhus I apprehend to be the lower manifestation of the disease; and in proportion as the scirrhous character predominates in a tumour—in proportion as the blastema has suffered itself to undergo a fibrous transformation—in such measure I cease to recognise that which is distinctively cancerous and malignant. In this respect I would compare encephaloid and scirrhus, in their mutual relations, to the fibre-forming and cell-forming sequels of inflammation; the intenser inflammatory acts running into the pure cell-forming process of suppuration; while the less intense inflammatory acts deviate into the slower construction of fibre.

I will not venture to deny that local conditions may check the development of cells, and may thus interfere with that free eliminative process, by which the cancerous cachexia seeks to relieve itself; and the disproportionate excess of nucleated fibrous material may sometimes possibly indicate some such local influence, thwarting the general tendency of the disease, and preventing the local structure from being an adequate expression of the constitutional fault. But—were it not for this possibility, I would venture to say universally (and even with this qualification, I would say generally) that the nucleated fibrous material will be abundant in proportion as the malignant cachexia has exhausted itself; that fibrous transformation, so far as it extends, illustrates the operation of common, not of specific, developmental influences in the part. So with regard to those compound granular corpuscles, constituting the yellow softening material in cancerous growth, there seems no difficulty in adopting the view (lately urged by Virchow) that these bodies represent the degeneration of cells, and consequently *cæteris paribus*, evince a retrogressive tendency in the effusion where they occur. If we may consider these two points established—that fibre, *quoad* its existence and quantity, illustrates an abortion of the cancer growth—that compound granular cells, *quoad* their existence and quantity, illustrate an exhaustion and retrogression of the morbid influence, we possess some material in pathology (though not always applicable in practice) for guiding our prognosis as to the probable recurrence of the disease. The lesser malignity of scirrhus, however, loses much of its relative advantages in fact, from the circumstance of its ready diffusion, by continuity of blastema, to the adjoining lymph-glands; and, as I have already told you, the presence of a single batch of cancer-cells in a lymphatic gland is quite sufficient to ensure for the cachexia every needful opportunity of development—quite sufficient to forbid the hope of any prolonged period of latency.

Next, as regards the inferences which may be drawn from the evidence of local causation in the origin of cancer,—there is a belief that blows often cause it, that it follows inflammation or functional excitement, that it invades erectile tumours, and the like. In regard of this belief, it appears to me that its preference for particular organs, or for organs in a particular state of action, admits, in some respects, of being generalised in a ready explanation. The part most readily selected for the invasion of cancer, the *pars minoris resistentiæ,* will probably, in many instances, be one previously the seat of hyperæmia. The uterus and the breast, with their frequent fluctuations of nourishment; the lip, or the tongue, or the stomach, which for years has been kept in a state of irritation and over-vascularity by local circumstances; the part which has suffered effusion from a blow; the vascular tumour, with its superfluous blood;—such are the organs in which cancer most readily makes its first appearance. And for this obvious reason, the disease has to develope itself out of blastema, furnished by the blood for the materials of its nourishment; it has to compete with the assimilative acts of the several healthy organs of the body; and it chooses, in preference, any structure where, by previous contingencies, the necessary materials for its growth may accidentally be present in excess.

As respects local influence, we may perhaps be justified in stating thus much:—that circumstances of topical irritation and hyperæmia will enable cancer to localise itself under a less intensity of cancerous cachexia than would else be necessary for the origination of the process; that, except for such local facility being afforded to it, the cachexia might have remained latent, or at least might never have realised its critical and evacuative tendency; that, for instance, a chimney-sweep, except for the local irritation of soot, would not have had cancer developed in its scrotum, and might not have had it developed at all. And in these cases, where we can distinctly recognise the influence of exterior irritation in promoting the evolution of the disease, and where (as occasionally happens) we can believe that the cachexia would have been insufficient for that effect, except for the adventitious aid thus given to its feebler intensity; in these cases and on these principles it is, that surgical interference is often eminently useful, by depriving the cachexia of the local advantage it had gained. It is peculiarly in respect of cutaneous cancer, and cancer of the tongue, that this line of argument applies; and I may mention to you, that, in these instances of the disease, there is a still further illustration of the low intensity of the cachexia, seeing that the formation of new structure is always here at its minimum; that the development of true cancer-growth is always peculiarly slight; and that here (if anywhere) it is, that the previously existing elements of the tegumentary surface are used for the eliminative action of the disease. For what is called epithelial cancer is, in regard of its function, as true a cancer as any; cancer of the lip or of the tongue is as unquestionably an evidence of the cachexia, though not in so high a degree as fungoid testicle would be; and yet, in its examination, we often are unable to discover any elements but such as we might ascribe to epithelial hypertrophy.—*Lancet, July* 27, 1850, *p.* 103.

4.—PRACTICAL INSTRUCTIONS ON THE REMEDIAL EFFICACY OF A VERY LOW OR ANÆSTHETIC TEMPERATURE IN CANCER.

By Dr. James Arnott, Physician to the Brighton Dispensary.

[From the report of a case of cancer, treated by an anæsthetic temperature or congelation, Dr. Arnott shews "that it furnishes us with a perfect means of relieving the pain of that dreadful disease, without producing the stupefaction and disturbance of the system that attends the use of narcotics, and that instead of precipitating the unfortunate patient's fate, congelation, by arresting the accompanying inflammation, and perhaps destroying the vitality of the 'cancer cell,' is not only calculated to prolong life for a great period, but may not improbably in the early stages of the disease exert a curative action." Dr. Arnott proceeds:]—

As the subject will probably be entirely new to many readers of this paper, it may be proper to give a brief account of the agent whose effects in cancer it is its purpose to describe. The very low or anæsthetic temperature that is used remedially as a local application to inflamed or painful parts, is produced by what are termed frigorific mixtures, or combinations of pounded ice and various salts, which, in dissolving reduce the temperature below the degree of zero of Fahrenheit's thermometer, or more than 30 degrees lower than any temperature hitherto employed in medicine. The application of such a mixture to the skin, or mucous membrane, causes little sensation of any kind, as the part soon becomes benumbed, and the slight tingling or smarting produced (which is seldom so great as to be complained of by the patient) is more allied to the sensation of heat than of cold.

In the first place, congelation will cure diseases and relieve pains that cannot be cured or relieved by any other known means; and where the same effect may be produced by other means, it is not produced so rapidly. Both as an antiphlogistic and as an anodyne, congelation is much more powerful as respects many diseases than any agent, or combination of agents, possessing similar qualities hitherto employed in their treatment, and which on that account have often proved ineffectual. A very low temperature will arrest every inflammation which is near enough to the surface to be accessible to its influence, and totally and permanently remove irritation from the nerves which it can reach.

In the second place, congelation is a safer remedy than most of those which are usually employed for the same purpose. Bleeding often impairs or prostrates the reparative powers; both antimony and mercury occasionally act as poisons; opium stupifies and excites; and events have shown that, as anæsthetics in surgical operations, ether and chloroform are not altogether without danger. Not once, in upwards of two thousand applications which have now been made of it, has congelation caused the least injury.

Other advantages of congelation might be mentioned, but these of its greater certainty, promptitude, and safety, must suffice. Cases are every day happening, where life is endangered or lost by inflammation that cannot be subdued by bleeding or the ordinary measures, without incurring greater hazard from the debility which they occasion, or other injurious effects; and cases of suffering, to which, from some constitutional peculiarity, the ordinary anodynes are inapplicable. That a great desideratum existed here, was strongly evinced by the recent deplorable case of a much lamented statesman, who died from injuries causing inflammation and intense pain. The medical art has never appeared to greater disadvantage than on that melancholy occasion. The inefficient measures resorted to, only showed the indications which the medical attendants were anxious to fulfil, but which, it would seem, they were unwilling to attempt fulfilling by the means in common use. What appeared to be wanting were, an antiphlogistic remedy that would not debilitate, and an anodyne that would not excite.

The patient, M. R., was admitted at the Brighton Dispensary on the 25th July, 1849. Of short stature, thin, sallow complexion, and about 42 years of age. Her principal symptoms were frequent, and severe paroxysms of pain, chiefly in the back and hips; a profuse and most offensive discharge and occasional hemorrhage from the vagina, and derangement of the digestive organs. On examination, the neck of the womb was found hard and ulcerated.

For six months the usual palliative treatment was pursued—viz., the exhibition of the preparations of opium and the application of leeches. She complained that the opium made her constantly drowsy and unfit for her occupation as a needlewoman; and the pain was notwithstanding its use, occasionally so severe as to oblige her to rise from bed and roll on the floor of her room.

In January, I determined upon a trial of congelation, having previously made another careful examination of the uterus. The disease had by this time considerably extended: the neck of the womb was now completely destroyed, and there were several warty excrescences in the upper part of the vagina. Congelation was effected by means of a frigorific mixture of two parts of finely-pounded ice and one part of chloride of sodium, introduced through a wide speculum of gutta percha, having the lower part of its upper opening of a cup-like form; and in order that the temperature might be maintained at the requisite low degree, or below zero of Fahrenheit, the dissolved ice was continuously drawn off by a syphon of peculiar construction. This peculiarity consists in a large two-necked bottle being connected with or constituting part of the long arm of the syphon; and the purpose of it is, that a stream of water may continue to flow along this part of the syphon, and keep up the suction at its proper extremity, notwithstanding any interruption in the supply. A tube of vulcanized india-rubber forms the remaining part of the syphon, with a small glass tube where it enters the speculum, in order that the rising column of liquid may be seen and regulated by a stop-cock.

The success of this application exceeded my expectation. So soon as I had learned to apply the frigorific properly, I was able to give immediate and entire relief, and this has generally continued complete for about a week. The discharge was soon diminished, and became much less offensive, and the tendency to hemorrhage ceased. From twenty to

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thirty applications of the frigorific have now been made, and scarcely any other remedy has been used. No advance of the disease appears, on examination, to have taken place, and in other respects there is decided improvement. The patient is not so thin; her appetite is tolerably good; she is stronger, and able to occupy herself in the usual household affairs.

She is directed to call whenever the pain returns. The speculum is generally introduced by herself while in the supine position, and she covers her extremities with a sheet before I enter the apartment. The nates are raised, in order that the speculum may be sufficiently upright to contain enough of the frigorific, which has usually been kept applied for a period varying from a quarter to half an hour. There is a slight sensation of smarting produced for a minute or two, and the pain from the disease has generally ceased within the first five minutes. If the womb be now inspected by removing the frigorific from the speculum, the greater part of its visible surface will be found perfectly white and hard. The application is terminated by allowing about a quart of cold water to run rapidly through the speculum and syphon for the purpose of gradually restoring the natural temperature, and washing away any remaining salt.—*Lancet, Aug.* 31, 1850, *p.* 257.

The benefit derived from congelation, in the case of uterine cancer just related, is not only very superior, but almost forms a contrast, to that usually obtained by other palliative means. Besides that the relief from pain is more complete and lasting than that obtained by the preparations of opium, it is gained without the stupefaction and sickness that generally attend the use of these; and instead of shortening life, by the disturbance of its functions, as these and other palliatives most unquestionably do, congelation will much prolong it, and prolong it in comfort, by arresting the course of the disease. Whether more is to be expected from it—whether, in the earlier stages especially, it may not possess energies equal to the complete removal of the disease—is a point that must be decided by further experience.

It has been a question, whether certain diseases, such as pulmonary consumption and cancer, which have been found to resist all the methods of treatment hitherto employed, are not necessarily fatal or incurable by the powers of nature or by art. As respects the remedial powers of nature, this question may, unless certain observers have been much deceived, be answered in the negative. Of cancer, at least, many natural cures have been recorded; and it is, moreover, in opposition to analogy, to suppose that any morbid condition of the body is necessarily incurable.

Another question, on which the opinions of inquirers have been much divided, and which bears closely on the efficacy of the local remedies of cancer, is that respecting the seat of the disease—whether this exists in the constitution as well as in the part manifestly affected. Late microscopical investigation, by Müller and others, has thrown considerable light upon this point, by showing that cancerous growths consist, in great part, of a congeries of peculiar cells. As these morbid cells, possessing inherent vitality, may be regarded as a species of parasitic animalcule or hydatid, the oldest hypothetical notion of the nature of cancer appears, curiously enough, to have received a foundation in the latest anatomical researches.

[The remedies that have been employed in cancer are empirical or rational. In this disease, however, empirical remedies are of no value. The rational practices may be divided as follow:—1 Means of subduing inflammation. 2 Narcotics. 3 Medicines which have been employed as alteratives. 4 Extirpation or ablation. 5 Pressure. And lastly, cold. It is upon the value and efficacy of this last remedy that Dr. Arnott particularly enlarges. He says,]

Cold has already been adverted to as a remedy for cancer in the consideration of the means best fitted to subdue inflammation; but it is calculated to fulfil other purposes. It is well known that a certain temperature is necessary for the maintenance or vigour of both animal and vegetable life.

"In a cancerous growth (says Prof. Bennett, in his recent and excellent work) the tendency of which is to excessive cell-formation, we evidently retard its advancement by the application of cold. Were it possible, indeed to bring down the temperature of an entire growth below the vegetating point, we must inevitably kill it; but supplied as it is through the warm blood within, this is impossible. Still the external application of cold is one of the most powerful means we possess of retarding the progress of a cancerous or any other growth."

As a means of applying cold in cases of external cancer, the same writer adverts, in favourable terms, to the apparatus alluded to in a note to the preceding paper, by which a low temperature can be uniformly and long maintained, and at the same time be conjoined with pressure. In the appendix to my Essay "On the Present State of Therapeutical Enquiry," I included this amongst other probably useful applications of the "current apparatus," and I have since had an opportunity of ascertaining its value in practice. The instrument is much more easily managed than at first might be supposed. The vulcanized caoutchouc tubes through which the current passes to and from the pressing water-proof bag, may be easily detached from the reservoir and waste vessel by the patient herself, if she wishes to rise from the sofa; and the current can be as easily re-established.

Having now passed in brief review the several rational measures which have been used or suggested in the treatment of cancer, we shall be better able to form a correct opinion of the mode in which congelation operates so beneficially as a palliative, and of the probability of its proving an effectual cure of this disease. As a palliative, it fulfils most completely the purposes of the first two classes of remedies just enumerated—viz., those possessing antiphlogistic and anodyne properties, and, unlike these remedies, it fulfils them without any counterbalancing injurious properties.

If cancer were essentially an inflammation, according to the doctrine of Broussais, there would be little doubt that congelation would, in many of its localities, prove a cure, for no inflamed part which can be put thoroughly under its influence will resist it. Or if the opinion of

Prof. Bennett be correct, that a "temperature below the vegetating point must inevitably kill cancer-cell," and if it be true that cancer-cell constitutes the disease, we might reasonably indulge the same hope. At all events, as congelation surpasses in power every local therapeutical agent which does not actually destroy the animal texture, we may safely, from this, together with other considerations, conclude that no remedial means hitherto proposed is, under any management, so likely to afford an effectual cure.

In estimating the effect of the application of frigorific mixtures to the animal tissue, it is highly necessary to take into account the condition of the part subjected to them, in respect to its own vascularity, and the vascularity of the neighbouring parts, and as to whether it be in a state of inflammation or not. A highly vascular or inflamed part necessarily requires, in order that the same effect should be produced, a much more powerful congealing agent than a part in the reverse condition. Although five minutes is the usual period of the congelation which I apply to the exterior of the body, when the skin and subjacent tissues are in their normal state, it was, as has been related, continued from fifteen to thirty minutes in the case of uterine cancer, and probably not with half the effect as when it is applied to the exterior, on account of the higher vascularity and natural heat of the part subjected to it. The difference between the periods mentioned was owing to the more or less perfect application, the time required to relieve the pain, and sometimes to the appearance of the congealed part.

Various modes of applying frigorifics to cancer must be employed for its various localities. That which has been described is perhaps the most complicated.* When the cancer is on the exterior of the body, the frigorific can be easily applied by the small net which I have already described in speaking of the use of congelation in neuralgia and diseases of the skin. As an illustration, I will very minutely describe its application to the case of cancerous breast, in the Middlesex hospital.

It was made on the 22nd of June last. The patient (an elderly woman, of the name of Pocock) had long suffered from severe and almost continuous pain. Her breast was ulcerated, but not much enlarged. About half a pound of ice having been put into a towel, and broken up into powder on the floor of the ward, by means of a flat iron, was quickly mixed in a jug with about half the quantity of common salt. The mixture was then poured into a small net of the thinnest silk gauze, and immediately applied to the breast, over a circular space of about four inches in diameter. The brine, as it trickled from the net, was absorbed by a moist sponge held underneath; and the net was occasionally raised, for the double purpose of inspecting the breast (as the surgeon who had kindly permitted the operation, and his assistants, were naturally anxious to watch the effect) and of stirring the mixture. The skin became white in a few seconds, and the congelation was continued for precisely three minutes. There was a slight pricking sensation produced for about half this period, which ceased upon the part becoming benumbed; and a similar sensation was experienced for some minutes after the congelation had ceased, although the greater part of what is usually felt at this time was prevented, by keeping the part covered for about five minutes with the ice net, after washing out the salt), and by the subsequent application of a rag dipped in cold water.

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* Cancer is not the only disease of the womb to which congelation may be thus applied with advantage; and by means of a frigorific mixture contained in a thin tin tube, of the diameter of the common speculum, and having its end closed and rounded, congelation can be conveniently used in inflammatory affections of the vagina.

I did not see the patient again until the 6th of July,—fourteen days after the application, -when she told me that there had not been the least return of pain. The sore on the breast was clean, and even healthy in appearance; and nothing but the common water-dressing had been applied. There had been no necessity for a repetition of the morphia, to which she had been so long accustomed.

In cases of this description, it would probably be wrong to wait for a return of pain as a sign for the re-application of the frigorific. The disease may advance without the accompaniment of pain; for it is well known that some cases of cancer are attended with little or no suffering. The congelation is demanded to stay the irritation, and the progress of the malady, as much as for the alleviation of pain.

In order that the frigorific mixture may be perfectly applied, it is necessary that the diseased surface should be in a horizontal position; and where it is denuded by ulceration, a thin membrane spread over it might act as an artificial cuticle, and prevent the smarting which the contact of the salt would otherwise produce before the production of anæsthesia.

Congelation need not be confided in exclusively. Several of the measures enumerated above may be used in combination; and equable and easily controllable pressure, whether by itself, or conjoined with cold, is, as respects many localities, one of the principal of these.

It is an important fact, that cancer in the womb, in which pressure can hardly be employed, should, in cases to which the operation is applicable, have (probably from the isolated nature of the prolapsed organ) been more successfully treated by amputation than cancer occurring in any other locality. The misfortune is, that the cases are rare in which this operation can be resorted to. If the womb be forcibly dragged out before the incision is made, or extirpated while in its ordinary situation, death (as ample experience has shown) is almost the inevitable and immediate consequence. Yet, when we consider that a womb which has become prolapsed from natural causes may be removed with very little danger, (a fact attested by numerous examples,) it becomes an interesting question, whether, in the early stages, and before the disease has extended to the adjoining parts, it might not, supposing all other means should fail in arresting the malady, be very gradually, and without irritation, brought into the same favourable position. If natural prolapsus of the womb mainly proceed, as is commonly supposed, from relaxation of the vagina and the external parts, this relaxation could be easily effected by the use of such a fluid dilator of equal pressure, as I have, on another occasion, suggested (and which I have myself employed) as a substitute for the forceps, or for excitants of uterine contraction, in cases where the birth of the child has been opposed by the most common of all causes of protracted labour—the unyielding of the external parts.—*Lancet, Sept.* 14, 1850, *p.* 316.

5.—*On the Treatment of Strumous Disorders by Means of Preparation of Leaves of Walnut.* By Dr. NEGRIER, Physician at Angers.—[Dr. Negrier has published two memoirs, the first in 1841, and the second in 1844 on this subject. These consisted entirely of practical facts, without any theoretical view either of the disease or remedy, and the experience of five years, and that of several foreign physicians, have given corroboration to the facts which the author then advanced. In this, the third memoir, he is desirous to avoid everything beyond a simple statement of facts, and the inferences which directly may be deduced from them. The following general conclusions are deduced by M. Negrier:]

Fifty-six patients, affected with scrofulous disease, in different forms, were treated by means of preparations of the walnut-tree leaves.

In this number, thirty-one were cures, and did not afterwards disappoint expectation. Eighteen patients, without being completely cured, underwent a degree of amendment very remarkable in their condition; and the greatest number among them were in the course of being cured. Four patients have obtained no advantage from the use of the medicine, from the state of their sores. Among them was one who was remarkably strengthened, and who, at the time specified, seemed in the course of being cured, under the use of cod liver oil.

Four children died during treatment; but two were destroyed by tubercular phthisis, one by acute *encephalitis*, and the fourth by double pneumonia.

The treatment, by the preparations of walnut-tree leaves, has furnished to him results, which he regards as sufficiently advantageous, in order to justify their serious examination, and warrant their being subjected to the control of systematic observation, particularly by hospital physicians, who have better means than others of administering the remedy accurately and in comparison with other agents.

Experience has satisfied him that the long-continued use of walnut-tree leaves has never caused, in the economy, any unpleasant effects. This medicine, which may be placed in the class of slightly-aromatic bitters, possesses an efficacy nearly uniform in scrofulous disorders.

The first effect of the preparations of walnut-tree leaves is to augment the activity of digestion and circulation; to all the functions they impart a remarkable degree of energy. A natural question is, whether they exert any special action upon the lymphatic system? The facts recorded by the author lead him to think that they do. Under the influence of their use, the muscles become more firm, the skin acquires a ruddy tint, and speedily parts with its chlorotic paleness. It may be a subject of inquiry whether the walnut-tree leaf contains any other principle than tannin, which is, doubtless, not foreign to the tonic action of this vegetable.

As to comparison between this and other received methods of treatment, the condition in which most of the patients whose cases formed the subject of clinical observation were found, at the time of treatment, sufficiently prove, he maintains, that these different modes of management possessed no efficacy, when recourse was had to the preparation of walnut-tree leaves.

The preparations employed are the following:

The infusions of the walnut-tree leaves are made by placing a good amount (pinch) of these leaves, cut, into two hundred and fifty grammes of boiling water. This infusion is sweetened with sugar, or with the syrup to be presently noticed. M. Negrier prescribed from two to three cupfuls of this infusion daily; and as many as five may be given.

The decoction of the leaves, which acts advantageously in the form of lotions, and as a topical application, in which the pledgets for dressing the sores are immersed, ought to be more strongly charged than the infusion. M. Negrier uses a small handful of leaves to a kilogramme of water, and boiling is continued from ten to fifteen minutes. This preparation is also very useful in the form of baths both local and general. Its efficacy is particularly remarkable when used by injection into fistulous passages.

The extract is prepared with the walnut-tree leaves, by the method of displacement. By employing the dry leaves, the practitioner has in his power the means of renewing this preparation, as often as he requires, during all seasons; while, by using recent leaves, it is requisite to prepare it in too large quantity, and it is liable to undergo alteration.

The syrup of the leaves of walnut-tree is prepared with the extract by mingling forty centigrammes with thirty grammes of simple syrup. By this means the practitioner knows what are the doses of the medicine which he employs. The syrup may also be prepared with the green leaves; and it is then more aromatic than that which is compounded with the extract; but it is impossible in this case to form a correct estimate of the quantity of the medicine which the patient daily takes.

To young children he gives two or three teaspoonfuls of syrup in the course of twenty-four hours. The ordinary dose for adults is from thirty-two to forty grammes; and he has never gone beyond sixty-four grammes.

The pills of the extract of walnut-tree leaves are each twenty centigrammes of extract, rendered consistent by a sufficient amount of the powder of the leaves. Of these, two are taken daily; and he has never gone beyond four daily.

Lastly, in some cases, in which it may be useful to cause friction to be made upon the afflicted region, he employed an ointment composed of thirty grammes of extract of the leaves, forty grammes of lard, and fifteen centigrammes of essential oil of bergamot. The frictions require to be made gently, and for about fifteen minutes twice daily.

It is easy to understand, he observes in conclusion, that, considering the nature of the disease, the salutary effects of treatment are sometimes slow in appearing. He therefore recommends perseverance; and he expresses the opinion that, if in the hands of some practitioners the preparations of walnut-tree leaves have been administered without efficacy, the reason is, that either the patient or the physician became too soon tired with using them. It is, in short, requisite to persevere, when it is remembered, that, in order to obtain a durable cure, the physician has not only to oppose the effects of the malady, but also to effect a thorough change in the constitution of the individual.

[Having published his second memoir in 1844, M. Negrier completed the history of those patients given in the first paper, adding a list of persons treated by the remedy subsequent to the publication of the first memoir, and he gives the following inferences on the result of the observations of six years' experience upon the mode of treatment:]—

1. Scrofulous disorders are in general radically cured by the use of preparations of walnut-tree leaves.
2. The action of this therapeutic agent possesses a sufficient degree of steadiness to enable the practitioner to reckon upon the cure of three-fourths of the patients treated by means of it.
3. The action of this mode of treatment is generally slow. It requires from twenty to fifty days, according to the nature of the symptoms, and the constitution of the subjects, in order that the effects be rendered sensible.
4. Patients cured by means of the preparations of walnut-tree leaves almost all retain the health which they have obtained under the influence of this treatment. Few relapses take place.
5. The effects produced by the internal use of the extract of walnut-tree leaves are at first general; the influence of this method of treatment is subsequently evinced upon the local symptoms.
6. In certain forms of scrofulous disorders, it is only after a long time that the efficacious action of this treatment is observed. This remark is particularly applicable to strumous glands not in a state of ulceration.
7. The preparations of walnut-tree leaves exert, on the contrary, a sufficiently speedy action upon the sores and the fistulous openings, whether they are maintained or not by caries of the bones, excepting in persons of dry and nervous temperament.
8. Till the present time, the instances of strumous ophthalmia which M. Negrier has observed, have been certainly and more rapidly cured by this method of treatment than by any other.—*Edin. Med. and Surg. Journal, Oct.* 1, 1850, *p.* 271.

6.—ON THE CONSTITUTIONAL ORIGIN OF ERYSIPELAS AND ITS TREATMENT.

By ALBERT J. WALSH, M.D., one of the Medical Officers of St. Peter's Dispensary, Dublin.

[The following are the observations of Dr. Walsh, founded on seven cases of traumatic, and fifty-six of idiopathic, erysipelas. He says,]

The predisposing cause of the disease seems to me to be a derangement of the liver and digestive organs, which gives rise to a peculiar kind of fever or constitutional irritation; this irritation nature attempts to get rid of by the erysipelatous inflammation of the cuticular surface. If the constitutional irritation be not sufficient to cause this inflammation, great anger, a blast of cold moist air, the application of any irritating substance to the skin, surgical operations, punctured wounds, ulcers, any abrasion of the surface, or other injury which in a healthy constitution would have either healed immediately or caused phlegmonous inflammation, will be sufficient to give rise to it. These appear to be the immediate causes of erysipelas. We find that this peculiar state of the constitution is most frequently met with in those addicted to drink malt and spirituous liquors, and lead intemperate lives; also in those who are sedentary and subject to great depression of mind. We find also, that the state of the air has the power of producing this peculiar kind of fever, for the affection is most frequent in autumn, or in seasons when hot weather is succeeded by cold and wet. Patients in hospital often suffer from this constitutional irritation, from breathing confined and bad air.

I have come to the above conclusion, that erysipelas is a peculiar kind of fever, or constitutional irritation, producing an inflammation of the skin, or skin and subjacent ăreolar membrane, for the following reasons:—

First. The disease, like small-pox, measles, &c., is subject to metastasis.

Second. There is always the same train of symptoms preceding and attending each case.

Third. A very minute dose of tartar emetic, which would have comparatively no effect in other inflammations, generally causes severe vomiting and purging in this.

Fourth. In mild cases, the disease will wear itself out; the part where the eruption first appeared getting well, while the disease is spreading to sound parts; and this continues until the system is completely rid of the poison which caused this peculiar fever. The patient then is in better health than for some time previously.

Fifth. It often recurs in the same patient at stated intervals; or if a patient who has had an attack lives irregularly, he is almost certain to suffer from a recurrence of the disease.

Sixth. That it is propagated by contagion will most probably be acknowledged, though I have myself seen but two instances; yet the observations of Lawrence, Copland, and others, appear to establish the point.

[Dr. Walsh divides the disease into idiopathic and traumatic. With regard to the treatment, Lane advises it to be treated on the purely antiphlogistic treatment, the most strenuous supporter of which is Mr. Lawrence: others, looking at it as a species of putrid fever, recommend tonics and stimulants, for which Dr. Fordyce is perhaps one of the strongest advocates. Dr. Walsh continues:]

Having mentioned the antiphlogistic and tonic methods of treatment, I shall now allude more particularly to the third plan, that recommended by Desault. He says: "In the bilious erysipelas, whatever degree of fever and heat may exist, I give, in the first instance, a grain of tartar emetic dissolved in a considerable quantity of fluid. The symptoms generally diminish as soon as the effects of the medicine have ceased. I have seen them entirely subside, although the medicine produced no other sensible alteration in the animal economy than an increase in the secretions of the insensible perspiration and the urine. Sometimes the symptoms resist the evacuations, and we are obliged to have recourse once or twice, or even more frequently, to the use of the emetic drink."—"When the erysipelas is cured, and the bitterness in the mouth and fever have subsided, two or three purges of cassia and manna, with a grain of tartar emetic, are exhibited. During the treatment the patient is ordered to drink freely of a diluting ptisan acidulated with oxymel. As soon as the symptoms are mitigated, the diet of the patient is enlarged; for when it is too rigidly observed the acrimony of the humours is apt to be increased, and the bilious erysipelas to be reproduced, particularly in a hospital, where the air, generally speaking, is unhealthy. I have invariably observed that the cases of persons who had been bled previously to their admission into the hospital were more serious and obstinate, particularly when it had been frequently repeated. The same practice is not applicable to the phlegmonous erysipelas; in this kind emetics and other evacuants augment the irritation and tension already considerable, nor should they be had recourse to till the plethora and irritation of the patient are diminished by one or more bleedings, according to the urgency of the symptoms and the strength of the patient. The bilious erysipelas that then appears points out the necessity for evacuants, and the proper time for their exhibition."

I have now alluded to the different modes of treatment recommended both here and on the continent. The first and second are diametrically opposed; and the last agrees with the first, with this exception, that in it is employed an agent which, in my opinion, may be called a specific. But Desault says we are not to bleed in the bilious erysipelas, but to depend on the tartar emetic; in the phlegmonous, on the contrary, we are to bleed even more than once if necessary, until we have reduced it to the bilious, and then we are to commence with the tartar emetic. Now in my opinion -an opinion borne out by the results of the sixty-three cases of erysipelas which I have treated on the plan described in this paper—there is no form of the disease which should not be attacked from the first with tartar emetic, whether there be high inflammatory fever, low typhoid fever, vomiting, or purging; and under all and every circumstance we shall find that the disease yields to this remedy. But I must be understood to say, that we are not to give this medicine in large doses, as recommended by Richter and other practitioners, who adopt his plan of treatment; for then it produces too violent an emetic effect, causing much general irritation, which being superadded to the irritation of the disease, the beneficial effect of the remedy is prevented; but I advise it to be given in very small doses as recommended by Desault, which may have to be repeated three or four times, so as not to allow the action of the medicine on the system to subside, for if it does we shall very likely find the disease to make more rapid progress after the first check.

Tartar emetic appears to me to act specifically in erysipelas for the following reasons, which depend on its sensible effects:—In most cases it vomits and purges after the second dose, or it vomits only, or it purges only; and what is vomited is green bile, and the same is passed by stool. I have known the second dose to cause from twelve to fourteen stools of bilious matter, with a decided remission of the symptoms. Now I have ordered this medicine in the same proportion in other diseases, as in phlegmonous inflammation, without the least perceptible effect on the patient; I have given it in bronchitis without any visible effect; and in other diseases I have administered it in the same proportion, and have found no such violent action as I have invariably seen even in the mildest cases of erysipelas.

If, from what I have now said of tartar emetic, it is to be expected to cure every case of erysipelas when administered alone, disappointment will ensue, as all that it seems to do is to remove from the system the morbid matter which appears to have caused the disease, and which nature was attempting to get rid of; as soon as that is done we must give the patient tonics, either wine or porter, strong broth, bitters, bark, ammonia, or quina: of all these I have found the sulphate of quina the most successful. It may be asked, at what time are the tonics to be given ? The general rule I would lay down is, that as soon as we find the erysipelatous surface to be getting a yellow tinge, and the skin shrivelling a little, the tongue cleaning, and the pulse, which generally falls under the influence of tartar emetic, becoming more frequent, the proper time has arrived to commence with tonics and omit the tartar emetic. In some cases where we have not so marked an improvement in the symptoms, and we are afraid the strength is failing, we shall have to commence the tonics, while we are still continuing the tartar emetic; and in other cases, where there are low typhoid symptoms, with prostration of strength, we shall have to give, with the tartar emetic, stimulants and tonics from the commencement.

While the patient is taking tartar emetic the bowels may be constipated, and we shall have to give some aperients: the saline aperients are what are generally recommended, but I usually order the compound rhubarb pill, with blue pill, in the proportion of three grains of the former to one of the latter, to be made into a pill; two of these to be taken for a dose, and, if necessary, to be repeated in six or eight hours. I prefer this combination, as it acts on the liver and large intestines.

[Passing on to the local means recommended in the treatment, Dr. Walsh says he is opposed to any local application, except in idiopathic erysipelas, in which flour or powdered starch may be used, but chiefly to gratify the patient, and in the traumatic form developing the part in a large linseed meal cataplasm. Discussing the other various local applications recommended, Dr. Walsh first mentions abstraction of blood by leeches, against which he says there is a great prejudice, as the disease often ensues from the bites; and he is opposed to the making of incisions in the part, both as to the smaller but more numerous ones of Mr. Hutchinson, and the long one of Mr. Lawrence, believing that, at least in the great majority of instances, the antimonial treatment, if early adopted, will obviate their necessity. Dr. Fahnestock, of Pittsburgh, recommends the local application of the purest creasote: of the effects of this Dr. Walsh cannot speak. Prof. Velpeau recommends a solution or ointment, made of sulphate of iron ; but considering all cases depending on some internal cause, Dr. Walsh does not believe it so available as the Professor supposes. The next local remedy mentioned is the nitrate of silver.]

Nitrate of silver has been recommended by Mr. Higginbottom, of Nottingham, to be applied over the entire of the inflamed surface. He says: "I have never in any case seen metastasis or any other bad effect from its use." He uses it in the following manner:—nitrate of silver, four scruples; nitric acid, six drops; distilled water, half an ounce: mix. This is to be applied several times on the inflamed parts, and for two or three inches beyond the inflamed on the healthy skin.

M. Piorry has recommended the use of linear blisters, by means of which he states that he has discovered a method of effecting the desired limitation of inflammatory action with great certainty. At the commencement of the disease he applies narrow blisters around the entire circumference of the inflamed skin, at the distance of one or two inches from its border. Nitrate of silver and solutions of sulphate of iron have been long used with the same view, but they have for the most part failed.

The only other application which I shall mention is mercurial ointment. This is to be smeared over the entire of the inflamed surface. It was first recommended by Ricord, and is still strongly advocated by some of our leading men,

The last is the only local application which, in my opinion, is worthy of consideration, as it acts not locally but constitutionally, for we know that in a very short time the system is affected by the mercury, and as soon as that takes place, the erysipelas gradually declines. But with respect to the other local applications, I consider that they are injurious without constitutional treatment, and with it unnecessary. As, if my ideas are correct, viz., that the erysipelatous inflammation is the method taken to throw off the morbid state of the blood, anything that tends to check that eruption will be the means of preventing the efforts of nature, and probably cause a metastasis to some other part of the body, it may be to a more vital organ, for we have on record many cases of metastasis of erysipelas to the brain, larynx, and pericardium.

As a summary of these remarks I would draw the following conclusions :

First. That erysipelas is a constitutional disease, depending solely on a morbid state of the blood; and that the eruption and fever are the means nature takes to get rid of this poison.

Second. That for all practical purposes, it is only necessary to divide the disease into idiopathic and traumatic.

Third. That tartar emetic seems to act specifically in erysipelas, by assisting nature in her efforts to throw off the disease.

Fourth. The best method of administering this medicine is by dissolving one grain in a quart of any bland fluid; the solution to be taken in the twenty-four hours.

Fifth. That as soon as the tartar emetic has acted sufficiently, sulphate of quina, or some other tonic, is to be administered.

Sixth. That if the patient is debilitated we must administer tonics at the same time that we give the tartar emetic.

Seventh. That under this treatment the erysipelatous inflammation may spread, but not with the same violence, nor to the same extent, as if the disease were left to itself.

Eighth. That we shall often require to give aperient medicine during the course of the case, as it is absolutely necessary to keep the bowels free.

Ninth. That local applications are unnecessary, and often injurious.

Tenth. That incisions are not necessary, except in the third, or suppurative stage; and if the antimonial treatment be early resorted to, it very rarely occurs that suppuration takes place.—*Dub. Quarterly Journal of Med. Science, Aug.* 1850, *p.* 50.

7.—*Rheumatic Fever Treated by Lemon-Juice.*—By R. HAMILTON, Esq., Surgeon to Charing Cross Hospital.—Some few months ago, a long discussion took place at the London Medical Society, on the value of lemon-juice in the treatment of rheumatism, and its efficacy was by many denied. In the following case, Mr. Hancock having tried various remedies, as colchicum, calomel and opium, &c., without much benefit, prescribed it, considering it a good opportunity for testing its value. It has been for the last year or two, given at Guy's Hospital, and is very highly spoken of by Dr. G. O. Rees and Dr. Babington.

Ellen Wilson, a servant, aged 22, was admitted July 1st into the hospital, under the care of Mr. Hancock, with chronic disease of the inferior maxillary bone of nine years' standing. She was in Guy's Hospital a few weeks back with a slight attack of rheumatism, but was not treated with lemon-juice. About eight days after her admittance, she was taken very ill with pains in all the limbs, coated tongue, flushed face, headache, and sickness. She was ordered the wine of colchicum with magnesia, which she continued for some days without any benefit, the rheumatism shifting about from one joint to another; at one time the knees and ankles being much swollen, and exquisitely painful; at another the wrists and elbows; the tongue being coated with a white fur, the face flushed, and perspirations profuse. There was also some affection of the heart, the *bruit de souffle* being heard distinctly, though she complained of no pain or uneasiness in the chest; the pulse was also intermittent. Her medicine was then changed to the calomel and opium, two grains of the former to a quarter of a grain of the latter to be taken every four hours. To have also the ung. hydrarg. fort. rubbed into the thighs daily, and a blister to be applied to the chest.

Under this treatment the chest-symptoms subsided, but the pains in the joints remained as bad as ever.

July 18th.—Mr. Hancock ordered half an ounce of the lemon-juice to be taken three times a day, and to discontinue the other medicines.

July 20th.—There is an abatement of all the symptoms, the pain in the knees decidedly less; the pulse 120 and full; perspires less; swelling diminished.

22nd.—Pulse 104, and weaker. Can raise her legs in bed; the tongue cleaner.

24th.—Continuing to improve; the pains in the limbs almost gone and is able to get in and out of bed without assistance.

From this time she continued to mend daily, and in about a week the lemon-juice was discontinued, and one grain of quinine, three times a day, prescribed.

Throughout the case, the urine was acid, and small in quantity; its specific gravity was not taken.—*Medical Times, Sept.* 14, 1850, *p.* 283.

8.—*Cases of Acute Rheumatism.* Under the care of Dr. BARLOW, at Guy's Hospital.—[The first case was that of a man aged twenty-six—strong and plethoric; and within the last three or four years having suffered from attacks of acute rheumatism, but cannot remember that the present one was brought on by exposure to cold and wet; hence from this case, the opinion, that when once the rheumatic diathesis is formed, the exciting causes of the attack are generally slight, receives an additional weight. On admission he had the various symptoms of rheumatic fever. But]

The heart's action being examined was found natural, with the exception of a slight systolic murmur heard over the interval between the fourth and fifth ribs, about an inch below the nipple. This patient having so repeatedly suffered from acute rheumatism, and being still young, it may be looked upon as probable that the pericardium was partly adherent, and that the murmur observed depended on valvular changes resulting from previous attacks of endocarditis. Still it is worthy of note that these symptoms were very slight, though at that age cardiac complications are very common. Dr. Watson says on that head—"The younger the patient is who suffers from acute rheumatism, the more likely will he be to suffer from rheumatic carditis."

The patient was now ordered six drachms of lemon-juice, to be taken in sugar-water, three times daily. He was, however, in a couple of days, much worse; the pains were very acute, and the joints along the arms and hands became involved; the pulse rose to 120, full; the systolic murmur was more distinct; the perspiration profuse, very acid, and the urine high-coloured. Dr. Barlow, after giving a purgative draught, with wine of colchicum, prescribed a pill, composed of a quarter of a grain of tartar-emetic, half a grain of opium, and one grain of calomel, to be taken every fourth hour. These measures contributed to lessen the pains, and the patient was desired to take the pills only three times a day, with the following draught:—half a drachm of acetate of potash, and ten grains of nitre, in camphor mixture. The improvement was very great on the next day; the pills were discontinued, but the mixture persevered in. The favourable impression produced by the salts of potash increased during the next few days; all the symptoms gradually gave way, and ten days after the draughts had been first taken, and regularly continued, the patient was convalescent. He took, before being finally discharged, small doses of iodide of potassium and carbonate of potash, and left the hospital quite well, about three weeks after admission.—*Lancet, Aug.* 24, 1850, *p.* 243.

9.—DISCOVERY OF CASEIN IN THE BLOOD.

By Dr. Panum.

[Dr. Panum ascertained on pouring some blood serum into a glass of water, that the mixture became whitish and opaque, like diluted milk and water. After performing various experiments to satisfy himself as to the true nature of this opaque deposit, it became evident, he says, that "being insoluble in water, ether, and cold alcohol, and being precipitated from its solution in acetic acid by ferro-cyanide of potassium, it possessed the character of a coagulated protein compound." He proceeds to say:]

The question arises, whether we can deduce any more accurate conclusions as to the chemical nature of this substance, beyond what we have already shown,—that it is a *protein-compound, insoluble in water?* Fibrin has hitherto been the only constituent of the blood, which could be asserted with certainty, to possess the power of passing from a soluble to an insoluble state, whether in or out of the body. It has hence necessarily arisen, that an unlimited amount of physiological and pathological importance has been ascribed to fibrin. The albumen of serum can be brought, when out of the organism, to pass from the soluble to the insoluble state by various methods, such as heat, alcohol, and most acids, provided they act in a sufficiently concentrated form. But the circumstances, under which albumen is coagulated out of the body, can scarcely operate within it. The substance which has been described, is precipitated by simple dilution with water, and by acetic acid so highly diluted, that it may easily be imagined capable of being formed in the body itself The manner in which fibrin coagulates in the form of filaments, the circumstances under which it passes from the soluble to the insoluble condition, and finally, the different manner in which it is affected by re-agents, separate it so essentially from the substance which is being treated of, that the latter will have, in future researches, to be described as entirely distinct. I think it necessary to insist on this, on account of the confusion which has arisen from the description of many different things under the term fibrin: and this has become so important, that most have mistaken the primitive signification of the word fibrin, which, on physiological and pathological grounds, there is good reason for retaining, while there is no cause for rejecting it on purely chemical grounds.

This protein-compound is essentially distinguished from the albumen of serum, by being precipitated by water and acetic acid, which cannot coagulate albumen. It can scarcely be the product of a transformation of albumen: for after the substance precipitated by water and acetic acid has been removed, the liquid still contains a large quantity of albumen: indeed this appears more plentiful on boiling, than before the addition of acetic acid and the removal of the precipitated material. Besides, after this substance has been separated by filtration, water and acetic acid may be added in any proportions, without producing more sediment; but if the material were derived from the action of water and acetic acid on the albumen of the serum, there would be no reason why the transmutation should cease, after a certain quantity of albumen had been changed into its insoluble substance.

There can be then no doubt, *that our protein-compound is distinct from albumen and fibrin, and exists in the serum in a state of solution with the salts and alkalies, but is separated when the saline matter is diluted, and the alkali united with acetic acid.* This can only be asserted of deutoxide of protein, albuminate of soda, or casein.

We may, at least provisionally, regard the substance as casein, as its behaviour with acetic acid hitherto seems characteristic of that protein-compound. The diagnosis has depended only on this, in those instances in which casein has been *occasionally* detected in the blood. Gmelin was the first, who thus found a small amount of casein in the blood, in some rare cases. Marchand next found it in some instances, both males and females. Finally Hünefeldt seems to have discovered it in cases in which the secretion of milk was arrested; but he does not state whether the precipitate formed by adding acetic acid was soluble in an excess, so that no certain conclusion can be drawn from his observations.

The reason why the constant presence of casein in the blood has hitherto escaped the notice of observers, appears from the circumstances already mentioned. Only when the amount of casein in the serum is very great, or the quantity of salts, especially phosphate of soda, is very small, can the casein be precipitated from undiluted serum by acetic acid. But when the proportion of saline matters to the casein is not so small, they retain the casein in the solution, even when it is separated by acetic acid from its combination with soda. It is only when the solvent power of the salts is weakened by dilution with water, that acetic acid can precipitate casein in any quantity; and even then the phenomenon escapes notice, unless very dilute acid be employed, otherwise the precipitated casein is re-dissolved in the excess of acid. When there is more casein in the serum than is combined with an alkali, so that some is held in solution by the salts in the serum, a portion is at once precipitated on the simple addition of water, and appears first as an opacity, and soon as a sediment. When it is not free, but united with an alkali, simple dilution with water produces no precipitate, until dilute acetic acid is added, which uniting with the soda, precipitates the casein as a substance insoluble in water.

The great importance which a material with these properties may possess, and very probably actually has, with regard to the healthy or diseased organism, render necessary a far more careful and extended series of researches on its chemical relations. Thus it will be of the first importance to discover a convenient method for performing its quantitative analysis, which has hitherto only been accomplished by an extreme dilution of the serum, and by the very careful addition of a highly diluted acid. It will also be important to determine whether carbonic acid alone can separate casein from its combination with soda. But the interest of the subject appears to me so great, that I have been led to publish my observations, imperfect as they are. It is, however, a point of the greatest interest with regard to physiology and pathology, that this material, which occurs in the blood, constantly and in notable quantity, can be acted on by re-agents, which are constantly in operation within the organism; and that, after having been precipitated by them, it can be re-dissolved by many different salts.

A number of important questions here arises; but I can only refer to a few of them.

The relation which this substance may bear to the processes of nutrition and cell-development, in health and disease, can scarcely yet be accurately determined. There are, however, some points which tend to show that this relation must be very important. The following are some of them: (1.) The method in which cell-development is generally performed, commencing, as is well known, by the formation of molecules in the clear mother-liquor. (2) The facility with which the casein is precipitated from the clear serum in the form of molecules, by re-agents which are constantly in action within the organism—a peculiarity which is possessed by no other of the known constituents of the blood. (3.) The agreement in chemical properties between casein and the cell-membranes of the blood-corpuscles, and of the globules of milk and pus; which agreement is so great, that several chemists have described these cell-membranes as a modification of casein. Finally, (4.) The remarkable faculty which casein possesses of inducing transmutation in organic substances may lead to a well-grounded suspicion, that it acts in the same way within the limits of the organism.

In the next place, it will be very interesting to point out the relative quantity of casein in the blood during lactation; and it will likewise be very important to point out the proportion which it bears to the other constituents of the blood, and to the ingesta.

In a pathological point of view, it will be highly important to observe what relation the amorphous molecular mass, which is found in notable quantity in nearly all pathological exudations and deposits, bears to this substance, which may be very well imagined to play a principal part in a great number of organic diseases. Perhaps also. the solvent and antiphlogistic action of saline medicines in many of these diseases will find an easy explanation, in their faculty of dissolving the deposits of this morbid material. In the meantime, it is not to *hypotheses*, but to new *researches*, that we are to look for a solution of the many important questions connected with this subject.—*London Journal of Medicine, July*, 1850, *p*. 685.

10.—*On the End and Final Condition or Purpose of the Red Globules of the Blood.* By Dr. HASSALL.—Everywhere throughout the solid constituents of the animal organisation cellular tissue abounds; it forms the basis of every texture and organ of the body. It is, therefore, scarcely to be wondered at that the opinion should have been adopted, that the globules which exist in such vast numbers in the blood, were to be regarded as the primary and even parent cells, out of which all the solid structures of our frame took their origin. This theory, to the mind of the earlier micographer, must have appeared very rational and seductive; and so great, indeed, is the plausibility with which, even in the present day, it is frequently invested, that it is still able to claim a few adherents.

If we regard with the utmost patience and attention the beautiful spectacle of the capillary circulation in any of the more transparent parts of animals, but especially in the tongue of the frog, we shall in vain look for the escape from their containing vessels of even a single red blood-corpuscle, independent of a rupture of those vessels. In a normal state, therefore, the blood-globules are never free, but are always enclosed in their own proper receptacles.

A communication, however, between the fluid contents of the blood-vessels and the tissues lying external and adjacent to them, is doubtless established, through the operation of the principle of exosmosis, whereby a slow exudation of the fluid fibrin of the blood is perpetually going forward. Now it is the opinion of most of the German physiologists, and it is the view best supported by facts, that this fluid fibrin is to be regarded as the true *blastema*, out of which all the different elementary tissues and structures of the body proceed; and this not by any power inherent in itself, it being, as respects the final form which it is made to assume, totally inert and indifferent, and which form is impressed upon it by a *vis insita*, or peculiar power and faculty belonging to each organ and structure of the animal fabric.

While the fibrin circulates in the blood it retains its fluid form; soon after the cessation of the circulaion, and whether within or without the system, it passes from the fluid state to the condition of a solid; now, on the principle of endosmosis, which has to be so often referred to in the explanation of numerous phenomena, in the solidifying power of the fibrin, and the *vis insita* of the different tissues, we recognise the chief and fundamental causes which regulate nutrition, growth, and secretion.

It would thus appear that the globules of the blood (the red globules are more particularly alluded to) are not to be regarded as either cytoblasts or primary cells, forming by direct apposition the solids of the body; and that, therefore, they do not express the last degree of elaboration of which the fibrin of the blood is susceptible.

Again, then, we have to ask ourselves the question, what is the end or final condition of the red blood-globules? Direct observation is wanting to aid us in the solution of this difficult inquiry, which, however, admits of an indirect reply being given:—we have seen that no means of egress from the blood-vessels is, under ordinary circumstances, permitted to the red blood-globules, and, therefore, we are driven to the conclusion, that, having performed the important function to which we have already alluded,—viz., that of carriers of oxygen from the lungs throughout the system, and of carbon from the latter back again to the lungs,—they become dissolved, increasing by their dissolution the amount of fluid fibrin circulating in the blood, and which is deemed to be the true *blastema.—Edin. Med. and Surg. Journal, July*, 1850, *p.* 183.

11.—*On the Non-Identity of Flesh and Blood Fibrin.*—Liebig has lately made some important observations on the properties of the fibrin of blood and of flesh substances, which have erroneously been considered to be identical. The two substances behave in very different ways when treated with water, acidulated with 1-10th per cent. of hydrochloric acid. Blood fibrin swells into a gelatinous mass,—corrugated by addition of acid,—recovering its gelatinous consistence when water is again added. Flesh fibrin, on the contrary, dissolves completely, the solution being rendered turbid only by some fatty particles, which can be separated by filtration; when neutralised, the fibrin coagulates to a thick white mass, soluble in excess of alkali. Common salt added to the alkaline solution produces a coagulum soluble in excess of warm water; the white mass produced by the neutralisation is also soluble in lime-water, and the solution, when boiled, yields a coagulum like a dilute solution of white of egg. In some kinds of flesh,—as in veal and mutton,—in addition to this fibrin, there is another substance, insoluble in water, acidulated with hydrochloric acid.

Blood fibrin contains more nitrogen than flesh fibrin, hence Liebig doubts whether it serves for the formation of the latter substance. Blood fibrin has other remarkable properties; thus, when completely immersed in water, it rapidly putrifies when the vessel is placed in a warm place. The fibrin in about three weeks dissolves completely, forming a slightly coloured solution, which is indistinguishable from a solution of albumen; not only does a coagulum form when heated, but the coagulated substance, as appears from an analysis of Dr. Strecker, actually possesses the exact composition of albumen ($C_{53\cdot9}$ $H_{6\cdot99}$ $N_{15\cdot58}$ $S_{1\cdot59-1\cdot45}$ $\text{Ash}_{0\ 28}$). After the coagulum has separated, an azotized substance remains in solution.

Liebig observes, that blood fibrin always contains iron, which can be found in the white ash left on incineration.—(Abridged from the *Quarterly Journal of the Chemical Society*, July, 1850, p. 188.)—*Medical Times, August* 10, 1850, *p.* 151.

12.—*On the Alkaline Nature of the Serum of the Blood.*—The study of the blood has, within the last few years, assumed the rank which it was entitled to hold in medicine; but the great difficulty of the subject has hitherto prevented the investigators from arriving at any very practical conclusions. One point, however, seems to be clearly made out, viz., that mere quantitative analysis of the blood's elements will never lead to any great discovery in pathology. This arises principally from the rapid manner in which certain elements of the blood in the living body appear to be transformed the one into the other. Thus it would seem that fibrin and albumen are really but modifications of one and the same principle; and, if this be the case, we shall have to determine, not the relative proportions of fibrin and albumen in a given quantity of the vital fluid, but the conditions under which one form may be transmuted into another.

In a learned memoir, presented at the last meeting of the Academy of Medicine, M. Cohen discussed many interesting questions connected with this subject. The following are the principal conclusions at which the author arrives:—

In a state of health:—1. The serum of the blood holds in solution a substance which subsequently separates into albumen and fibrin. The albumen and fibrin may be transformed, one into the other, by artificial means. 3. When fibrin is treated with a weak solution of soda at 40°, it loses its properties, and is changed into a substance which it is impossible to distinguish from albumen.

In inflammatory affections:—4. The fibrin is increased, the albumen diminished. 5. The increase of the former is equal to the diminution of the latter. ·6. The quantity of the salts of soda is diminished. 7. On adding a small quantity of a solution of soda, we re-establish the normal proportions between the albumen and fibrin. 8. The serum of the blood is less alkaline. 9. The increase of fibrin depends on the diminished alkalinity of the serum.

In putrid diseases:—10. The serum is more alkaline than natural. 11. The fibrin is diminished, and this diminution depends on the increased alkalinity of the serum.

From the above we can deduce the theory put forward by M. Cohen. Fibrin is nothing but a normal excess of albumen in the blood, which the alkalies of the serum, saturated with albumen, are unable to dissolve. A diminution of the alkaline principles of the serum will, therefore, increase the quantity of albumen; or increase of the alkalies will diminish it, and *vice versâ* for the fibrin. The practical deductions to be drawn from the observations, if they be correct, are that alkalies are indicated in inflammatory affections, and acids in putrid ones.—*Medical Times, July* 27, 1850, *p.* 98.

DISEASES OF THE NERVOUS SYSTEM.

13.—ON THE NERVOUS SYSTEM.

By Dr. Marshall Hall, F.R.S., &c. &c.

[In some of our previous volumes we have repeatedly referred to the views of this eminent physician on this interesting subject. During the present year he has published a "Synopsis of the Spinal System," which we will take the liberty of making use of in order to circulate more widely such of his opinions as may appear to us to throw light on the practical departments of our profession. Dr. Hall first commences by explaining certain terms which are frequently used in his treatise. The following anatomical relations are essential to the understanding of his views:]—

A nerve leading *from* the point or part irritated, *to* and *into* the spinal marrow;

The spinal marrow *itself;* and

A nerve, or nerves, passing *out of* or *from* the spinal marrow,—*all in essential relation or connexion with each other.*

On these anatomical facts I have ventured to institute a new nomenclature, descriptive of what I have hitherto designated *The Spinal System,* and expressive of these essential points. The term peristaltic (from περι and στέλλω, to contract) is familiar to us all. It may be justly extended to all the movements of the interior organs, as the heart, the stomach, the large and small intestines, the uterus, &c. These movements, it is well known, are independent of the spinal marrow. But it has been shown that a series of experimental phenomena, and it will be shown hereafter that a series of important functions, are effected by means of the series of nerves in essential connection with the spinal marrow, to which I have adverted. The action or act is performed *through* the spinal marrow as its essential centre. I propose to designate the phenomena by the term *dia-* staltic.

The spinal system may henceforth be designated—*The Diastaltic Nervous System,* a designation which will have the advantage of including this system in the invertebrate as well the vertebrate tribes of animals. This system embraces a peculiar anatomy, physiology, pathology, and therapeutics.

Perhaps the only *purely* diastaltic function is *Respiration;* and this is variously modified by volition and influenced by emotion. But there are many other functions which partake of *both* the diastaltic and peristaltic character. Such are the functions of the immediate conduits of ingestion and of egestion;—the œsophagus, the rectum, the uterus. These functions are *dia-* and *peri-* staltic.

We are much in need of other terms still, to aid us in this investigation. The terms incident excitor and reflex motor have been used to designate those nerves whose influence proceeds *to* and *from* the spinal marrow. But they have never appeared to me satisfactory, and I have long wished for others more expressive and explicit. The following compounds of ὁδος, a way, have appeared to competent judges very appropriate to our subject: esodic (εσω) will express the action *into;* exodic (εξω) the action *out of;* anodic (ανω) will express the *ascending,* cathodic (κατα) the *descending,* course of action; pollodic (πολυς) and panthodic (πας) will express the facts, on which I shall shortly have to dwell at considerable length, of the action of the *vis nervosa* from each *one* point of the diastaltic system, in *many* or even *all* directions, to *every other.*

[Dr. M. Hall, in the third section of his work, first makes some general observations on]

THE ANATOMY OF THE DIASTALTIC SYSTEM.—In treating of the *Anatomy* of the diastaltic system, it is my intention to display, not the results of dissection by the scalpel, with or without the microscope, but of *experiment,*—not mere tissues, but organs and phenomena.

The great, the important division of the nervous system, is, into its in-excitor or astaltic, and its excitor and staltic portions. The former embraces the cerebrum and cerebellum, and the olfactory, the optic, and the acoustic nerves; the latter, the tubercula quadrigemina, and the new *Class* of the esodic nerves, the spinal centre, and the exodic nerves.

All this is but the expression of fact, the result of experiment and observation, all hypothesis being excluded.

Volition is seated in the in-excitor portion of the nervous system. The seat of emotion is so too, but approaches the excitor.

Whatever the *precise* limits of the different parts of the nervous system may be, there is no doubt that volition is seated higher in that system than emotion, and emotion than pain.

The hemiplegic attack, which severs the influence of volition from the arm, leaves that of emotion on this member entire; and it frequently happens that the patient is a prey, as it were, to uncontrollable laughter or tears.

Even when the whole encephalon is removed in an experiment on the rabbit, the sole medulla oblongata excepted, cries, the unequivocal expression of pain, are induced by pinching the foot or the tail severely with the forceps. And as the sense of touch is seated in the cerebrum and cerebral nerve of touch, so I think there are reasons to believe that the susceptibility to *pain* resides in the *medulla oblongata* and the system of *ganglionic* nerves. These reasons will be given hereafter.

In the case of the atrophy or defective development of the brain in idiots, the passions and appetites are developed in proportion as the intellect is defective.

Below perception and volition, then, emotion and passion are seated; below these are seated excitability and diastaltic action—the diastaltic system. Evenly with both the latter, we have the peristaltic and secretory system. The influence of emotion is shed over the spinal and the ganglionic systems.

There is the same *kind* of evidence for the nerves of each of these systems,—dissection, and experimental fact. No one doubts that the optic is the nerve of sight; as little can any one doubt that the trifacial is an excitor diastaltic nerve.

The late Sir Charles Bell treated of what he designated a "Nervous Circle," defining it thus:

Between the brain and the muscle there is a circle of nerves; one nerve conveys the influence of the brain to the muscle, another gives the sense of the condition of the muscle to the brain.

I think this view an error. I believe we have no consciousness of the condition of individual muscles, or sets of muscles, as we have no power of acting on such muscles. The sense, sometimes denominated the muscular sense, is, I believe, not in the muscle, nor in any sentient muscular nerve, but in the nerve of touch, or of vision; and volition is not directed to any muscle, or set of muscles, but to the aim, object, and purpose of their contraction. We are guided in our voluntary actions, not by muscular sense or nerve, but by the sense of touch or of vision, by the *cutaneous* or the *optic* nerves.

There is a class of phenomena in some cases of paralysis, which, I think, throw a ray of light on this subject. If there be loss of sensation in the fingers, the patient lets any object held by them escape and fall, unless the eye be continually directed towards them, to regulate the force of muscular contraction. If a patient have lost the power of sensation in the feet, he cannot walk in the dark; the eye is essential to the due action of the muscles of the lower extremities. I have a patient afflicted with partial paraplegia, who has no power of *balancing* himself without the aid of the eye. We have only to consult our own consciousness, to be aware that "the nervous circle is not between muscle and muscle, but between one sentient nerve, whether of touch or of sight, and another. The sensation does not ascend from the muscle, nor does volition descend to it; but the former arises from a nerve of sense—of special sense—and the latter is directed to an aim or purpose, in attaining which the former is the *guide*."

[He next makes the following remarks on]

The Spinal Marrow.—The first opinion respecting the spinal marrow was that still retained by Unzer—that it is a mere chord of cerebral nerves. The cerebrum and spinal marrow are still designated the cerebro-spinal axis, as being the common centre of the cerebral system.

Legallois treats of the spinal marrow vaguely, as possessing the "prerogative" of being the seat of *sensation*, and, at the same time, of the *life* of the trunk, and of its regions respectively. The former opinion is an error; the latter, unconsciously on the part of this admirable author, sets forth its influence as the centre of the ganglionic system.

M. Flourens was the first clearly to distinguish between the cerebrum and the spinal marrow as in-excitor and excitor—a distinction of the utmost moment in physiology and pathology, as in diagnosis.

Legallois first singled out the medulla oblongata in its important and essential connection with respiration.

The next step in the progress of neurological science was that suggested by Walker, taken by Sir C. Bell, and confirmed by Professor J. Müller. Sir C. Bell was of opinion that the posterior columns of the spinal marrow and the posterior roots of the spinal nerves are destined for *sensation*, whilst the anterior columns and roots are subservient to motion.

Legallois first called the attention of physiologists to the distinct influence of distinct *segments* of the spinal marrow over the corresponding segments of the trunk. Mr. Mayo and others have also taken this view of the subject.

Such was the condition of our knowledge of the anatomy of the nervous system when I entered on its investigation, in 1830—an investigation which led to an entire distinction of the spinal system from the cerebral and the ganglionic, distinct at least in its application to physiology and pathology.

It was during this investigation that I discovered a *special nervous arc*, consisting of an esodic and anastaltic nerve, *essentially linked* with a special portion of the spinal marrow, and *through* this with an exodic catastaltic nerve and *special muscles.*

It was during the continued investigation of this subject that I observed that this nervous arc is not simple, but very multiplex, and that it is not one exodic nerve merely which is associated, through the spinal marrow, with the esodic nerves, but that nerves exodic in *all* directions are so associated.

The course of excited action may be traced along each and all of these *nerves*. But in what part of their tissue, and along what particles of the spinal marrow, their influence extends, we are utterly ignorant.

As each esodic spinal nerve serves for sensation and for conveying diastaltic action, it has been supposed that it contains distinct *fibres* for each of these two offices. And as the same exodic nerve conveys the impulse from volition and diastaltic action, it also has been supposed to contain *two* appropriate sets of fibres. The same opinion has been extended to the columns of the spinal marrow.

Experiment alone can determine such questions definitely; pathology does not afford such distinct isolation of tissues, so as to establish their special and exclusive functions; and, in the present instance, I fear that even experiment is inadequate to the task.

It is during the *progress* of diseases of the nervous system that our knowledge of its physiology is useful, by enabling us to *interpret* the symptoms, and to determine the *Diagnosis.*

The conclusions at which I have arrived may now be stated in a few words:

1. Within the spinal marrow there is a *special Nervous Centre;*
2. To this centre are essentially attached certain *special Esodic and Exodic Nerves;*
3. These together constitute a system of *Diastaltic Nervous Arcs;*
4. The whole of these, taken together, constitute the *Spinal or Diastaltic Nervous System,* viewed anatomically.

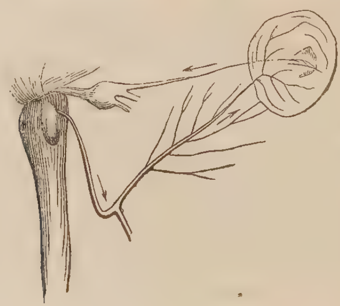
PHYSIOLOGY OF THE DIASTALTIC SYSTEM.—*The Diastaltic Nervous Arc.*—The anatomy of the diastaltic system consists in an esodic nerve, the spinal centre, and an exodic nerve, *essentially linked together,* and constituting a *diastaltic nervous arc.*

The physiology of this system consists in such an arc, or such arcs, in diastaltic action.

An appropiate stimulus, applied to any part of this diastaltic nervous arc, induces muscular contraction. In the *physiological* relations of this *arc,* the stimulus is always applied at its incipient esodic portions, or *origins,* in the cutaneous or mucous surfaces. In its pathological relations, the source of irritation may be situated in any part of this arc, origin, centre, or termination.

I here adduce three examples of diastaltic arc, for the sake of clearness.

I. THE DIASTALTIC ARC OF THE IRIS.

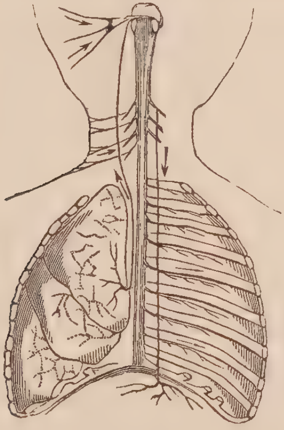


I. *The Excitor.*	II. *The Centre.*	III. *The Motor.*
1. The Excitor portion of the Optic Nerve. 2. The Trifacial.	The Tubercula Quadrigemina	The Motor Oculi

II. THE DIASTALTIC ARC OF THE EYE-LID.

I. *The Excitor.*	II. *The Centre.*	III. *The Motor.*
The Palpebral branch of the Trifacial.	The Medulla Oblongata.	The Orbicular branch of the Facial.

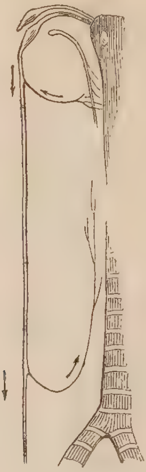
III. THE DIASTALTIC ARC OF RESPIRATION.



I. *The Excitors.*	II. The Medulla Oblongata.	III. *The Motors.*
1. The Trifacial, 2. The Pneumogastric, 3. The Spinal, Nerves.		1. The Intercostal, 2. The Diaphragmatic, 3. The Lower Spinal, &c.

Of all the diastaltic nervous arcs, that by which the larynx is closed is the most simple, whilst that by which deglutition is accomplished is the most complex.

The Diastaltic Arc of the Larynx.



I. *The Excitor.*	II. *The Centre.*	III. *The Motor.*
The Superior Laryngeal.	The Medulla Oblongata.	The Inferior Laryngeal or Recurrent.

The upper part of the pneumogastric nerve is thus diastaltic, and both esodic and exodic, in reference to the spinal centre.

The Diastaltic Arc of Deglutition.

I. *The Excitors and Motors which* close *the Larynx.*

II.

I. *The Excitors*—	II. *The Centre— of Deglutition.*	III. *The Motors*—
1. The Glosso-Pharyngeal? 2. 1. The Pharyngeal— 2. The Œsophageal— 3. The Cardiac—branches of the Pneumogastric.	The Medulla oblongata.	1. 1. The Pharyngeal— 2. The Œsophageal— 3. The Cardiac—branches of the Pneumogastric.

III. The immediate Excitors of Peristalsis in the Œsophagus.

IV. The Excitors and Motors which open the Cardia.

This complexity will appear the greater, when we reflect that, in deglutition, the act of closure of the larynx is added to that of the contraction of the pharynx and the œsophagus, and to that of relaxation and opening of the cardia.

A similar state of complication of action occurs in the uterine system and its auxiliaries in parturition, and, in a slighter degree, in the evacuation of the rectum, the expulsors being excited and the sphincters relaxed, simultaneously.

But in *respiration* there is, besides the excited act of inspiration, a sustained and continued influence by which the thorax and diaphragm and the abdominal muscles are kept in a state of equilibrium, as it were, in every stage of the double act of respiration—inspiration and expiration; so that, in the *physiological* condition of this function, nothing is abrupt, nothing laborious, nothing audible even, but all is accurately rhythmic and equable. How different, in cases of pathology, is the varied, irregular,—*diagnostic* condition of this important function.

All this is effected through those *diastaltic arcs and acts* which I have attempted to describe, and which now take their place in the science of *Anatomy* and *Physiology* for the first time.

Guards of the Orifices and Exits.—Besides the acts of *ingestion* and of *egestion*, of which I have briefly treated in the last section, there are other acts or actions by which certain objects are *excluded* from, or *retained* in, the system.

The contraction of the iris and the closure of the eye-lids exclude too intense light; that of the larynx excludes certain irritant and noxious gases. The sphincters retain the contents of certain organs, the cardia those of the stomach, the sphincter ani and the sphincter vesicæ those of the rectum and bladder.

In this manner certain esodic nerves become the *guards*, as it were, of certain organs. These I have thrown into the form of a *Table*; thus:

I. *The Trafacial guards*—
  1. The Eye.
  2. The Nostril, the Ear, in the Cetacea.
  3. The Fauces.
II. *The Pneumogastric*—
  1. The Larynx, the Bronchia.
  2. The Pharynx, the Cardia.
  3. The Ureter, the Gall-duct.
III. *The Spinal Nerves.*
  1. The Rectum.
  2. The Bladder.
  3. The Vesiculæ Seminales.
  4. The Uterus.

[Thus Dr. M. Hall considers that the diastaltic system, amongst other functions, especially *guards* the *borders*, the *orifices* and *exits*, the *sphincters* of the body. On the *interior* of these borders, this system is in general *tubular*, and in these parts of it the action is not purely diastaltic. Thus, according to Dr. J. Read,]

If the pneumogastric nerves be divided above the origin of the superior laryngeals in the rabbit, the principal part of the parsley eaten by the animal remains in the œsophagus; a few leaves are all that reached the stomach. On the other hand, I myself observed that, if the œsophagus, thus replete with food, be placed on the table for observation, that food is slowly and gradually expelled by peristaltic action.

The act of deglutition in the pharynx and œsophagus is, therefore, *dia-* and *peri-* staltic.

The same remark doubtless applies to all the *tubal* acts of ingestion and egestion, and especially to those of the rectum and the uterus. The *degree* in which these acts are diastaltic and peristaltic respectively is, as yet, unknown.

[The whole subject of *peristaltic actions* is reserved by Dr. Hall for a future investigation, and is not entered upon in this work. Conception and parturition are also principally diastaltic spinal acts, but neither does Dr. Hall enter minutely into this branch, but refers us particularly to the investigations of Dr. Tyler Smith.]

THE PATHOLOGY OF THE DIASTALTIC SYSTEM.—[On this subject Dr. Hall justly considers that a knowledge of the diastaltic system is one great means of advancing the pathology of the diseases of the nervous system.]

The cerebrum and cerebellum are astaltic, or inexcitor. No disease *limited* to these can, therefore, be attended with *spasm*.

The tubercula quadrigemina are excitor with a *crossed* effect.

The medulla oblongata and the medulla spinalis are staltic or excitor, *without* crossed effect. *Irritation* of these organs induces spasm, on the side affected.

The dura mater and other internal membranes are diastaltic; that is, they excite muscular actions *through* the medium of the spinal centre. Hence, dental, gastric, intestinal, or uterine irritation may excite diastaltic convulsive disease.

Dental irritation, acting through the spinal centre, in which a morbid degree of excitability is induced, is seen in its effects on the muscles of the eyes, the fingers, the toes, inducing strabismus, and spasmodic flexion. The features, the neck, and the limbs, are distorted in succession. The larynx is closed, and there are morbid efforts of the expiratory muscles. The consequence is, compressed veins of the neck, and congested encephalon.

Similar phenomena are observed in the adult, from gastric, enteric, uterine irritation, the affection receiving, wrongfully, the formidable designation of epilepsy. That of epilept*oid* would be more just and more appropriate.

Similar phenomena occur, constituting the various forms of puerperal convulsion.

Lastly, *similar* phenomena take place from organic lesion within the cranium—a case to be carefully distinguished from the former.

THE DIASTALTIC SYSTEM IN RELATION TO DIAGNOSIS.—It is by the absence of these phenomena from the beginning that we determine the limitation of disease of the cerebrum to that organ, or its extension, in influence or actual lesion, to the spinal marrow.

I may mention hydrocephalus in infants, and apoplexy in adults, as examples of this fact. In the commencement of the former, the symptoms are cerebral and obscure; but, as the disease advances, staltic or spasmodic actions *denote* and almost *measure* the fact. In apoplexy, the respiration is frequently stertorous, and there may be dysphagia, or there may be various convulsive affections of the limbs. If these symptoms subside. we may conclude that the disease originates in cerebral congestion, which is receding, and with it the immediate danger; but if they remain permanent, we must fear that the disease is formidable organic lesion, and will prove fatal.

I have already observed that diseases of the cerebum, *limited* to the cerebrum, cannot induce *spasm*, this organ being *in*-excitor. But, in the case of congestion or other lesion, there may be tumefaction of its structure, and counter-pressure on the medulla oblongata, and consequent affection of the larynx and throat, the muscles of respiration, and the muscular system variously, spasmodic or paralytic.

A tumour, or exostosis, within the cranium, may irritate or compress the spinal structures. The former condition leads to spasm; the latter, to paralysis. The *base* of the cranium presents an object of study to the physician, of extreme interest.

If, in the case of paralysis, there be also *spasm*—if it be *spasmo*-paralysis, two inferences may be drawn: the first, that the spinal system is affected; the second, that it is *irritated*, not *destroyed*,—a diagnosis of extreme importance in the prognosis and treatment.

If, in the case of paraplegia, diastaltic actions remain in the affected lower extremities, two inferences may again be drawn: first, that the seat of the disease is so high as to leave a lower portion of the spinal marrow; secondly, that this lower portion of the spinal marrow is intact.

If paraplegia comes on very slowly, the great question again is—Is there, or is there not, *spasm?* In the former case, it is irritation; a mild mercurial course has appeared to me to be the remedy: in the latter, it is probably *exhaustion;* and I have seen great benefit from the fiftieth part of a grain of the acetate of *strychnine*, given three or four times a day.

Epileptic and Epileptoid Diseases.—But the great distinction to which we are led by a knowledge of the phenomena of the Diastaltic Nervous System, is in regard to—I. *The Esodic;* II. *The Centric;* and III. *The Exodic, Diseases* of that system.

Let us take epilepsy as an example. Every one knows the almost hopeless case of epilepsy of intra-cranial origin. Such is the force of *language*, that the same ideas are attached to those forms of epilepsy, or rather those epilept*oid* affections, which arise from irritation of organs distant from the nervous centres, acting through esodic nerves. This important *Diagnosis* is to be effected by carefully tracing the symptoms to their *source*, and by further tracing the influence of that source and cause of the disease upon its varied forms.

The attacks of centric epilepsy are generally more sudden and more formidable than those of epilept*oid* diseases arising from *ex*-centric causes.

Formidable as the convulsions arising from teething, or occurring in the puerperal state, may be, no one attaches to them the *same* idea which is attached to epilepsy of centric origin. The same difference of idea ought to be attached to the epileptoid affections arising from gastric, enteric, or uterine irritation.

In the latter case, the malady is frequently not only traceable to an *ex*-centric source, but suspended by appropriate remedies. The diagnosis points at once to the proper principles of treatment; and this diagnosis is founded upon the physiological principles which have been laid down.

It would occupy too much space to give even aphoristic expression to all these topics, as I have hitherto attempted to do to the other subjects of this little volume. I will only add a specimen of the manner in which the *links* of this *chain* hang together.

I will again take epilepsy as an example, and sketch the precise mode of action of the causes of epilept*oid* diseases:

They all act diastaltically through the spinal centre, and induce strabismus, distortion of the features, a bitten tongue, contraction of the hands and fingers, or toes, or both; spasmodic action of the muscles of *The Neck*, the cleido-mastoid, the trapezius *(trachelismus)*, &c. compressing the internal jugular and the vertebral veins *(phlebismus)*, and inducing congestion of the features, brain, and medulla oblongata; this state is augmented by closure of the larynx and expiratory efforts; insensibility, general convulsion, coma, &c. are the dire results; and, more remotely, perhaps paralysis, perhaps mania.

Few maladies have been so traced, link by link. I will only further illustrate this part of *Diagnosis* by adding the following

Table of the Epileptoid Seizure.

I. *The Esodic or Excitors.*	II. *The Centre.*	III. *The Exodic or Motors.*
1. The Pneumogastric in the Stomach. 2. The Spinal in 1. The Intestine. 2. The Uterus.	The Medulla Oblongata	1. The Recurrent, inducing Laryngismus; 2. The spinal Accessory, the Descendens noni, &c., inducing Trachelismus; 3. The Spinal, inducing 1. Forcible Expirations; 2. General Convulsion, &c.

Such Tables, illustrated by Diagrams, might be drawn out, illustrative of all the diseases of the nervous system.

THERAPEUTICS OF THE DIASTALTIC SYSTEM.—[In the treatment of diseases of the diastaltic system one principle prevails throughout, viz., *the stimulus of an esodic diastaltic nerve.*]

The most *useful* of these measures is the sudden application of cold water. Dashing cold water on the face, or on the chest, induces a sudden inspiration. The sudden application of a cold douche on the abdomen induces contraction of the uterus, in cases of hæmorrhagy.

But there must be *difference* of temperature, and the phenomenon ceases on repetition, if the part itself becomes cold. In this case, I imagine that the application of water of somewhat elevated temperature would have a good effect. I have myself experienced that to descend into a cool bath, and into a bath of from 100° to 104° Fahr. is equally attended by sudden inspirations.

In the case of asphyxia, such a principle ought to guide us; but experiments on the subject are entirely wanting.

There is one fact of great interest in this matter. It seems that the course of the effect of each stimulus is panthodic. Dashing or applying cold water on the face induces, not only an act of inspiration, but an act of deglutition, and the evacuation of the bladder, the rectum, and even the uterus.

Mr. Simpson observed that if, in coma, fluid were placed in the mouth and remained unswallowed, an act of deglutition was instantly excited by dashing cold water on the face. Dr. Tyler Smith observed, in a case of faintishness from uterine hæmorrhagy, that if cold water were dashed on the face, the uterus instantly contracted on his hand within its cavity.

This panthodic action is a general principle, and deserves to be fully investigated.

Dashing cold water on the thighs and abdomen sometimes induces the evacuation of the bladder or rectum. It is instructive to know that a *similar* effect is induced by the easier method of dashing cold water on the face. I think that in some cases of difficulty in passing the catheter, from spasm, this difficulty and this spasm might be removed together by dashing cold water on the face; and that, in some cases of constipation, a similar effect on the rectum might be produced by the same measure.

[On *Electricity* as a therapeutic agent, Dr. Hall observes,]

The effect of the electro-dynamic apparatus is unlike any thing in Nature! It is that of a most irritant alternation of current.

There is a question, in reference to the application of electricity or galvanism to the animal frame, of the most intense interest. A current cannot pass without inducing an electrogenic state. This, in muscle and humid nerve, is a state of less susceptibility to the *same* current, but of unimpaired susceptibility to the *opposite* current. The current may be applied and changed, with this effect, repeatedly. It has been designated the "alternative Voltaique," from its discoverer, as seen in the lower animals. Its existence in the human subject I have myself observed. The fact must be taken into account, in judging of the effect of galvanism as a *test* of irritability.

A similar remark applies to the effect of repeated contraction of the muscle from *any* cause. Its irritability is diminished for a time. This fact must also be borne in mind, in employing galvanism as a *test* of the irritability of the muscular fibre.

What is the most probable good or therapeutic effect of galvanism? It may excite the failing *heart* in asphyxia or syncope, and the *uterus* in inertia of this organ. It may *exercise* the nervous and muscular system, and so restore them, in some measure, from atrophy and debility. It is, in *this* manner, useful in the paralysis remaining after the organic cause of the disease may have been removed. It may be useful in paroxysmal paralysis, in paralysis of the facial nerve from cold, in some hysteric forms of paralysis, &c.—*Synopsis of Diastaltic Nervous System.*

[In the above paper we have endeavoured to place before the reader the most interesting remarks of Dr. Hall, which are scattered over a volume of 100 quarto pages. We advise the reader to procure the whole treatise for himself.]

14.—ON THE PHYSIOLOGY AND PATHOLOGY OF THE GANGLIONIC NERVOUS SYSTEM.

By James George Davey, Esq., Licentiate of the Royal College of Physicians, &c.

[In the preceding notice of Dr. Marshall Hall's treatise on the nervous system, little or no reference is made to the ganglionic system of nerves. The following paper by Dr. Davey may fill up this desideratum; and although a long one, it is, from its novelty and originality, well worthy of attentive perusal, notwithstanding the startling views which it explains.]

"The object of the following paper," says Dr. Davey, "is to prove that *Life* is the function of the *Solar Plexus*, regarding it as the *root* of the ganglionic or sympathetic system. That the *Solar Plexus* is the *impetrum faciens* of Hippocrates, or the *materia vitæ* of Hunter; that it is the *organ* whose *function* may be represented as the principle or stimulus which enables every other and subordinate part in the animal economy to continue its specific and allotted labour towards the existence of the individual; that both the brain and spinal cord, in common with all the viscera, hold a similar relation to, and dependence on, the *solar ganglion*, as the centre of the ganglionic system, that the iris does to the retina, or the external senses do to particular parts of the cerebral mass; and these opinions the author has privately circulated for the last *ten* years—that is, since 1835—as is well known to many medical friends and acquaintances. I am aware that Dr. Stevens has also advocated some such views; but it cannot be doubted that the *priority* is not with him."

Introductory remarks on the Nisus Formativus, or the Solar Ganglion.—The physiologist, if I mistake not, will consider that my position, as explained in the advertisement to the reader, must derive no inconsiderable confirmation from the circumstance, that the solar ganglion is that particular portion of our organism which is first formed in utero, and therefore may be really considered as the *germ* of all the phenomena to be afterwards developed.

Nothing can appear more reasonable than that that portion of our organism, from which every other takes its character and vitality, should enjoy a prior existence. The egg precedes the chick, and the specific vitality of the former is impressed on the latter. The foundation is erected before the house, and the design of the architect, it may be added, is not unfrequently to be anticipated by an early examination of it. Müller says, in his Physiology, translated by Dr. Baly, that "Ackermann asserts that the sympathetic nerve is the part first formed in the fœtus." Rolando moreover declares what has been usually considered as the first traces of the vertebræ at the sides of the spinal cord, in birds, to be the ganglia of the sympathetic nerve. The assertions of Ackermann and Rolando acquire great weight from the testimony of such men as Blumenbach and Gall; both of whom add the authority of their illustrious names, and confirm the former-named physiologists in their opinions. Blumenbach says, "The nervous system," meaning the organic nervous system, "of the chest and abdomen, are fully formed, while the brain appears still a pulpy mass," and refers to Gall's writings for the same views. He adds: "These ganglia and nerves would hardly be formed before the brain and spinal marrow, but for the sake of the organs which they supply, and the functions of which (with the exception of the genitals) are as perfect at birth as at adult age, while the mind and brain are slowly perfected."

Ackermann, Rolando, Blumenbach, and all, all maintain, then, as I have shown, that the ganglionic or organic nerves of the abdomen and thorax are the first formed in the embryo. Now it is extremely unlikely that the development and formation of the solar ganglion –the centre and source of the organic nerves and their anastomosing branches—should be preceded by "*organic nerves;*" their dependence on it, it may be said, is analogous to the dependence of the branches of a tree on its root; and I therefore would claim for the solar ganglion a like precedence. Viewing the matter in this light, it is readily seen that the solar ganglion itself is, in the embryo, nothing more nor less than the *nisus formativus* of Blumenbach, and that, like it, it excites, even in its rudimentary existence, the elaborated fluids of the successful coition, and like it, it vivifies and shapes the hitherto shapeless spermatic matter partly into the beautiful containing ovum, and partly into the contained embryo.

The *nisus formativus*, we are told, occur to the genital matter, when this is mature, and committed to the uterus in a proper condition, and under proper circumstances, produces in it the rudiments of conception, gradually forms organs fitted for particular purposes, preserves this structure during life by nourishing the body, and reproduces, as far as it can, any part, accidentally mutilated.

The "nisus formativus," says Dr. Elliotson, in a note, "produces a being generally resembling the parents, but occasionally different." It is understood, then, that exactly what Blumenbach and Elliotson, in common with other physiologists, claim for the nisus formativus, I claim for the solar ganglion. I cannot doubt that it exercises the *architectural* power which is employed in man and animals—from man downwards, through the whole of animated nature to the very lowest link in the chain of being; that to its peculiar and vital influence must be conceded, upon the grounds before stated, the wonderful and successive metamorphoses or changes which characterize, not only the intra- and extra-uterine existence of the human form, but also that of animals, whether oviparous or viviparous, and under circumstances both of normal and abnormal action. No one, I think, would presume to explain the *modus operandi* of this power or influence of the solar ganglion; it is sufficient for my purpose that it exists. This question is involved, equally with that of the *cause* of gravitation or attraction, in a too Cimmerian darkness for my optics to penetrate.

If, from any cause, the organic affinities in progress in the fœtus be interfered with—if the balance which must obtain in the distribution of the imponderable matters in the organic tissues be disturbed, the action of the organism and of the separate organs may be so modified as to give rise to the formation of some one kind or other of monstrosity. An injury done to a seed during its germination is seldom unattended with ill effects. The radicle or plumule will, the one or the other, suffer, and perhaps both. The injury done may be confined to one of these parts, and yet it may very seriously affect the vital principle in operation, and so modify or aggravate the original mischief. It is just so with the fœtus; any abnormal change in the component matter of any of its parts may prove irremediable, and the effects of which being then communicated to the solar ganglion, may excite so altered a vital action in it as to prove incompatible with the complete or normal development of either itself or of those parts dependent on it; and hence the existence of monstrosities, of acephalous, and other malformed children, among whom the physical characters of the biped are more or less substituted by those of beings lower in the scale of creation. "Human monsters," says Blumenbach, "are not unfrequently met with who strongly resemble the form of brutes;" and it is, "because the '*nisus formativus*,' having been disturbed and obstructed from some cause or the other, could not reach the highest pitch of the human form, but rested at a lower point, and produced a bestial shape." He adds, strangely enough —"On the contrary, I have never once found among brutes a true example of monstrosity which, by a bound of the *nisus formativus*, bore any analogy to the human figure."

It may be asked, in the history of monstrosities, did any physiologist ever hear of *one* in whom there was no ganglionic system, no *solar ganglion*? I answer, no! Such an occurrence is wholly impossible. We have all heard of acephalous children, and of beings created without either *brain* or *spinal cord*. What, in such instances, sustained intra-uterine life? what enabled the body of the creature to be nourished and developed? What, I ask, was the source of all this vitality? The solar ganglion and its branches, the ganglionic nerves! "In fœtuses, without *brain* or *spinal marrow*," says Blumenbach, "the circulation, nutrition, secretion, &c., proceed *equally* as in others, which, besides spinal marrow, nerves, and ganglion, possess a brain," and for the same reason, he might have added, that "after the removal or destruction of the *brain* and *spinal marrow* in animals, the heart still continues to act. and the blood to circulate, provided respiration is artificially supported." The *reason* is just this, the *solar ganglion* and its immediate dependencies are unaffected. With these *facts* before us, then, I must confess I am in ignorance of the "*experience*" which, "shows that when the influence of the brain and spinal marrow is intercepted, although the afflux of blood may for a time continue, yet the secretion ceases, and all the functions dependent upon secretion, such as digestion, cease likewise." We are informed, too, by the same author, strangely enough, that "the functions of digestion, circulation, absorption, secretion, and all those included under the class of nutrient or vital functions, are carried on as well during *sleep* as when we are awake," that is, as well during the total *inaction* of the *brain* and *spinal marrow*, when the "*influence*" of both must of course be "intercepted." Strange *experience* this.

Dr. Marshall Hall, in his Lectures, published in 1842, "On the Pathology and Treatment of Nervous Diseases," evidently favours the opinion that the influence of the brain is necessary to the complete performance of the vital or ganglionic functions. "We find," says Dr.

Hall, "that idiots with small brains are short-lived;" therefore "the animal functions cannot go on permanently independent of the brain." I take it that the imperfect development of the brain of the idiot must be regarded, only, as an indication of the mal-organized condition of the entire nervous system, including the spinal and ganglionic, not less than the cerebral.

In the very interesting case of acephalous monstrosity published by Mr. Lawrence, in which the brain *only* was wanting, we learn that all the excito-motory functions were duly performed; it gave evidences of pain, and "at first moved very briskly," and the sphincters performed their office. Such, of course, was referrible to the integrity, generally, of the spinal cord. "The child's *breathing* and *temperature* were natural; it discharged *urine* and *fæces,* and took food." The latter set of circumstances indicated the operation of the functions of the solar ganglion and its collateral parts; *and without any aid from a brain.* The ganglionic and spinal nervous systems were natural, and they exercised their respective functions accordingly.

In the remarkable case cited by Dr. M. Hall, of a fœtus born "without either brain or spinal marrow, without a particle of either of these organs *yet perfectly developed,*" we have a very satisfactory proof of the independence of ganglionic functions on either the spinal or cerebral nervous systems. The intra-uterine life may be quite perfect, although there may be neither brain nor spinal marrow; but what happens the moment the child is born? asks Dr. M. Hall: "It cannot breathe; it cannot live an instant." It dies truly, but not because the brain or the spinal marrow, or both, are directly essential to the continuance of the function of the solar ganglion, but because that central organ, in the absence of the motor nerves commonly supplied to the respiratory muscles, is deprived of one most material agency or power with which it, the solar ganglion, is enabled to perform one of the many indispensable functions required by the animal economy. The blood cannot, under such circumstances, be decarbonized, and the circulation of diseased blood paralyzes the vital energies of every part of the organism. The effect is the same if a person with his cerebro-spinal and ganglionic systems entire, be made to breathe carbonic acid or any other irrespirable gas. Life, then, in such a case of monstrosity, cannot be said to cease from the want of any direct *cerebral* or *spinal* influence. The vital actions in the lower classes of animals which have no brain, and in those, too, which have neither brain nor spinal marrow, are not less completely performed than the same in man. Secretion, circulation, digestion, &c. are as elaborately and efficiently executed in the polypus and oyster as in man, and their breathing apparatus is more simple, and independent of any addition to the *organism,* whereby, in man, the nature of his dependence on, and relations to, the external world are explained. His responsibility then came to be understood, and his real position as a moral being to be justly appreciated.

The experiments of Dr. M. Hall, as given in his published Lectures, appear to me to be anything than satisfactory. He introduces the subject thus:—"Until very recently, we viewed the contents of the spinal canal as a cord of cerebral nerves, and the origin of a part of the ganglionic system. Now, it is very possible to remove the cerebrum, the centre of the cerebral nerves, and the *ganglionic system*, and yet leave *another kind* of nervous influence remaining in the animal body. I shall take this early opportunity of showing you a simple experiment. You see here an animal (a frog) from which the head has been separated, and of course I need not tell you that with the head the brain has been entirely removed; all the viscera have also been removed, and with the viscera every portion of the ganglionic system: Now I beg here to repeat, the cerebrum, the centre of the spinal cord of nerves, and all the ganglionic, have been removed from this animal, and yet, when I pinch the extremity, it moves so as to be obviously perceptible at the remotest part of this theatre. Thus, as I said before, we have here removed the centre of the cerebral system, and the entire ganglionic system. The brain, which we know to be the centre of all the sentient and voluntary nerves, has been removed, the ganglionic system has been removed, and yet you observe something remains. Now, gentlemen, that which remains I venture to call—in contradistinction from what has been termed a cord of cerebral nerves, and the origin of the ganglionic system—the *true spinal marrow*. It is plain, in the first place, that it is not a mere cord of nerves; if it were a mere cord of nerves, you might divide it, and then you would intercept its influence. But, if you observe here, this influence passes not only from one extremity to the other, but it also passes from the one set of extremities to the other set of extremities; thus, it is quite plain that there is a nucleus of nervous matter between the two anterior extremities, and another nucleus between the two posterior extremities by which these nervous links are united and associated in their motions one with another. Having thus, then, clearly laid before you the distinction which I wish to insist upon —namely, that there is not a division of the nervous system into two parts only, but into three, pervading all the different parts of the whole animal frame, I shall venture to term them the cerebral, the true spinal, and the ganglionic systems."

That Dr. Marshall Hall is perfectly right in considering that the contents of the spinal canal do *not* constitute a cord of cerebral nerves, and the origin of a part of the ganglionic system, there can be, to my mind, no doubt; but I cannot allow this opportunity to pass by me without claiming for the illustrious Gall the honour of being the *first* to render this anatomical fact clear and distinct. *He* it was who demonstrated that the spinal marrow only *communicates* with the brain.

Dr. Hall has endeavoured, as it appears in the preceding extract, to prove by experiment on the frog, that on the removal of the viscera with the solar plexus and its ganglia, the spinal cord will still continue the excito-motory functions, the head (and of course the brain) being at the same time dissevered from the body, and which circumstance of course, would prove, as the doctor affirms it does, "that there is not a division of the nervous system into *two* parts, but into *three*, pervading all the different parts of the animal frame." I have on many occasions performed the experiment on the frog as detailed by Dr. Marshall Hall, but *I* have invariably found that the removal or destruction of the ganglionic system of nerves is fatal to the *life* of the animal. The vital or "*true spinal*" phenomena, in that case, continue no longer than the peculiar contractile irritability of the heart or extremity of the animal after the removal of either from the trunk; proving, therefore, most clearly, that the original power possessed by the spinal cord to perform its peculiar functions is derived from the ganglionic system. If the animal be *not* decapitated, the same result follows the removal or destruction of the solar ganglion, and which could not be the case if the influence of the *brain* were directly required by the spinal marrow. My own experiments, then, prove not only that there are *three* distinct nervous systems united in man—viz., a cerebral, a spinal, and a ganglionic, but also that both the first and the second are dependent on the third—that they derive not only their very existence and integrity from it, but also perform their respective functions in virtue only of the influence they receive from it, and that they are, as it were, employed by it to establish our dependency on, and relationship to, the external world, of which man forms a part.

The *cerebral* and *spinal* systems of nerves together perform the animal functions, which, in the words of an eminent physiologist, prove us feeling, thinking, and willing beings; they are the actions of the senses, which receive impressions of the brain, which perceives them, reflects upon them, and wills; of the voluntary muscles, which execute the will in regard to motion; and of the nerves, which are the agents of transmission: the brain is their central organ. But the ganglionic system of nerves, with the solar plexus for its central organ, performs the vital or organic functions, or rather, supplies to each viscus the power which enables it to perform its specific functions in the animal economy. Secretion, nutrition, exhalation, and absorption, being, then, under its immediate influence and control, it must preside equally over the brain as the stomach, equally over the spinal cord as the liver. In short, the vital force of the solar ganglion, the centre of the ganglionic system, holds the same relation to the whole organism that *steam* does to the several parts of an engine. The said "*vital force*" and "*steam*" are equally the motive power, and it may be said, that to their different states or conditions must be attributed every kind of change, however slight, of which either the animal organism or the machine itself is at any time, and under any circumstances, susceptible.

The observations already made in reference to acephalous and other monstrosities, no less than those which relate to the experiments of Dr. M. Hall and myself, abundantly prove the physiological position I have here taken—viz., that *life*, regarded as the "assemblage of all the functions, and the general result of their exercise," has its immediate principle neither in the brain nor in the spinal marrow, nor in any of the viscera of the chest or abdomen, but in the *solar ganglion;* yet it is no less certain, that all these parts or organs are necessary to the maintenance or continuance of life, as it exists in man, and the great mass of the lower classes of animals. The brain, principally, because the mechanical phenomena of respiration seem to depend upon it;—the spinal marrow, because it exercises a guardian power over the acts of ingestion and egestion;—and the viscera of the chest and abdomen, because they are necessary to the formation and circulation of the blood.

The only way that I am enabled to account for the discrepancy stated in the experiment of Dr. M. Hall and myself on the frog, is, that the excito-motory action which was produced by pinching or pricking the extremities of the animal, after the removal of the viscera and the ganglionic nervous system, must have resulted from the influence of that remaining nervous principle which exists, for a longer or a shorter period, in any portion of the animal organism, after, even, its removal from the trunk or body to which it originally belonged. It is well known that Le Gallois, Prochaska, and Hunter, taught that the nervous power is generated throughout the whole extent of the nervous system, even to the smallest nerves, and that it can exist, *for a certain time*, in the nerves of any part, independently of its source; and there can be, I think, no doubt of it. I have seen the heart of the *shark* contract vigorously, for even many minutes after its removal from the animal,—a fact which proves, in the words of Hunter, that the nerves of a part continue the same action which they receive.

From the preceding observations it will appear that the "*formative power*" of Blumenbach, or the *materia vitæ* of Hunter, or the *solar ganglion*, call it by what name we will, is no less universal than the animal organism itself; and there can be no doubt that, like the animal organism, it observes corresponding varieties and gradations of form. It could hardly be expected that the "*formative power*" of the dog is similar to that of the man; or that of the fish, of the reptile; and so on, through the whole of living things. The germ and its product must possess qualities common to both. It is impossible, perhaps, to demonstrate the *exterior* or physical characters of this "formative power" in man or in animals, peculiar either to the same or to different species of each genus; yet is there an abundance of authority to show that such a specific difference must everywhere exist. The formative power, or the solar ganglion, regarded as the germ of all to be afterwards developed, may be said to represent, in common with the cerebral and other parts, in its successive changes in *utero*, its various and permanent conditions as they exist throughout the animal kingdom, beginning with the polypus, in which every vital action is conducted upon the smallest scale, by the least refined methods, and with the strictest economy of means; its apparatus being the simplest, the agents employed the fewest possible, and its various operations being carried on in one and the same place; proceeding in the mollusca, and in worms and insects, in which relations are exhibited to surrounding objects, and in which animals the senses and voluntary motion gradually make their appearance, the organic apparatus necessary to the exercise of these functions being superadded; ascending through fishes, reptiles, birds, and quadrupeds, in which the powers of sensation and motion become much more energetic, much more active, the internal life at the same time more and more developed, and the cerebral functions more and more numerous and diversified; and ending with *man* himself, in whom, as Blumenbach observes, the successive imposition of cerebral matter has reached its maximum; so that the summit of the nervous system, which corresponds with the forehead and vertex, is much larger in him than in any brute, and his intellect and moral feelings are proportionally greater.

Apropos, the comparative cerebral development of man and brutes is, as is well known, employed to mark the rank any one of either kind may hold in the scale of creation and intelligence; and very properly so; but it appears to me that the physiologist takes cognizance only of an *effect* on the development of the cerebral organism, which has resulted from the operation of a cause which he has hitherto failed to appreciate. Nothing can be more true than that "the higher we ascend, the more parts exist above the *medulla oblongata*, till, rising from fish and reptiles, through the numerous warm-blooded brutes—all distinguished by the relative magnitude of each cerebral part—we arrive at man," the "*summit of terrestrial objects;*" and it is equally true to my mind, that the successive increase of parts above the medulla oblongata is attributable to the operation of a preliminary cause—viz., the *Solar Ganglion, the source of all animal life, whether cerebral, spinal, or organic.* Surely if the ganglionic system be deemed either necessary or competent to preside over the vital actions necessary to the perfect development of the amyencephalous monster of Dr. M. Hall; if the secretion, nutrition, circulation, &c., as carried on in it, in virtue of the said ganglionic system, are sufficient for its growth and maturity, and for the perfectibility of its several organs, as the liver, spleen, heart, &c., and the consequent exercise of their functions, why should we doubt its powers to preside over the vital actions necessary to the entire fœtus? If the ganglionic system be sufficient to develope and mature a liver, a heart, and a spleen, and to excite them to the exercise of their appropriate functions in the animal economy, there is no reason why it should *not* do the same for the brain and spinal marrow. There certainly is much reason, as I trust has been shown, to feel satisfied that it does so; and that, moreover, to the modified operation of the solar ganglion must be referred all the varieties of life, psychical and corporeal, which everywhere abound.

To proceed:—On tracing the animal organization from above downwards, we observe generally a gradual diminution or simplification of parts, and which in every case is proportionate to the wants and adaptations of the animal. The decreasing cerebral organism, for instance, marks the downward progress of the animal in the scale of intelligence and feeling. This is seen throughout the vertebrated animals, as the mammalia, birds, reptiles, and fishes. The insect tribes generally can hardly be said to have more than a mere rudimentary brain; they have certainly a medulla oblongata, in which the nerves of the external senses take their origin. The spinal system, so far, is perfect in all its parts, and all the strictly vital functions are in full operotion. In the annelida, however, we mark a very considerable falling off: the absence of everything like a brain, and of the external senses, leaves the animal to exercise only a strictly spinal and ganglionic existence. The excito-motory function, in itself, seems perfect, and like it, the organic functions appear on a par with those of the mollusca. The only advantage of the annelida is probably in its means of progression. One more step downwards, and the ordinary medusa is seen a mere mass of living gelatinous matter, without the least indication of even a spinal life, and nevertheless executing the organic functions of assimilation, digestion, secretion, circulation, nutrition, absorption, &c. This last step of the ladder constitutes the first of the intra-uterine life of the fœtus; and it may be said, that just so many steps as it takes the physiologist to trace the successive and organic changes from man to the medusa, the same are required to trace the fœtus through its various metamorphoses during its intra-uterine existence; and there can be no doubt, as has been before observed, that each diminution or addition of parts, both in man and animals, is but the visible effect of a corresponding variety and gradation of the *nisus formativus,* or the *solar ganglion,* the *germ* of all to be afterwards developed.

> "First chain of being, which from God began,
> Nature's ethereal, human, angel, man,
> Beast, bird, fish, insect, what no eye can see,
> No glass can reach, from infinite to thee:
> From thee to nothing." POPE.

It may be added that the possession of the *three* nervous systems in man and in the higher order of animals does not pre-suppose that those which have only *two* or even *one,* have been insufficiently provided for. The organism of the polypus and the oyster is as nicely adapted to their wants and the circumstances which surround them, as is the organism of man.

In many instances among the lower classes of animals it appears that nature has intended so to economise her means that one part shall serve the purposes of two or even more, and in others she has so concentrated her forces that it is really astonishing. The reproduction of lost parts in the lobster and crab, and many other crustacea; and the extraordinary activity of certain of the external senses in some animals higher in the scale of organization than the crustacea, almost make one doubt on which side to consider the advantages! A division of labour is generally regarded as indispensable to perfectibility, but among the lowest classes of animals we find the opposite principle in operation;—thus the solar ganglion in certain animals executes the functions of both the spinal cord and the cerebrum; for in the absence of the brain and spinal cord, it is occasionally seen that both sensation and voluntary motion exist in the zoophytes. In the manner in which the infusoria and actiniæ pursue their prey, and in their selection of it; in the facility with which they recede from whatever may prove hurtful to them, and turn aside when they encounter one another, together with the highly sensitive and irritable nature of the organism of the medasuriæ generally; "we can hardly fail," as Dr. Roget truly observes, "to recognise the evidence of voluntary action." Herein we get at the nature of animal *instinct,* and which, in the absence of everything like a cerebrum and spinal cord, must be regarded as a specific function of the ganglionic system. Dr. Roget says, in his chapter on the "Comparative Physiology of the Nervous System," page 538, "But whatever may be their extent, it is probable that the *sensorial operations* of the zoophytes take place without the intervention of any *common* centre of action," meaning thereby that the *sensorial operations,* so-called, are performed independently of a brain. The same may be said of the mollusca and articulata. There are times, moreover, when the *vertebral animals,* as fishes, reptiles, and birds, including the mammalia, and even *man*, perform actions of an instinctive and intellectual character, and that, too, *without the intervention of any common centre of action.* What is the course of that instinctive agency which determines the young among the several classes of animals above enumerated, including man, to seek each its particular means of support. In what originates the very keen *choice* which is displayed by all of them for that which nature has so especially predestined for their respective uses? What determines the peculiar habits and mode of life of any one of them? The immature condition of the brain in the infant being renders it perfectly impossible that it can exercise any, even the slightest, influence in the matter; and no one could venture to affirm that the spinal cord took any part in it. There can be no *reason* to doubt, but, on the other hand, every *reason* to feel assured, that to the same cause of the "*sensorial operations*" of the zoophytes, and of that peculiar instinct which enables the lower classes of animals generally to provide, not only for all the necessaries of life, but even to guard against contingencies and anticipate difficulties, whether they relate to the kind of habitation, the mode of progression, or to the kind of food required for their sustenance; in a word, to the same cause, the formative power or solar ganglion, which so beautifully adapts their individual habits, pursuits, and inclinations, to their peculiar organic conditions, and so providently harmonizes the natural laws, must also be referred whatever of *instinct* is, at any time, manifested in *man*, including the vertebrated animals.

The sudden and peculiar shrinking of the *hydra* when under the influence of fear, and the extreme caution and dexterity displayed by the infusory animalculæ in avoiding obstacles of any kind while swimming together in myriads in a single drop, are *instinctive* vital actions, arising from an inherent preservative principle derived from the solar ganglion, and similar in its nature to that which induces even us, in the *moment* of danger and doubt, to place our extended palm across the præcordia—thus the affected Miss, though ignorant of physiology or pathology, and perhaps of all other *ologies*, if either alarmed, or professing to be so, at any sufficient or insufficient cause of personal danger, quickly applies her hand to the præcordia; as if the solar plexus screamed "*take care of me now.*" What more reasonable than to expect that that organ, the sum of whose function may be in one word described as *life*, should preside over actions of the kind mentioned above, so *indispensable as they are to both its integrity and well being!*

The *instinctive* and *mental* (cerebral) faculties are occasionally seen acting in combination—*e.g.*, the martins which, in a spirit of retaliation and vindictiveness, built up the hole which had access to the nest that certain sparrows had robbed them of, and so buried alive the predatory occupiers in a grave of their own seeking, afford an instance of a clear and distinct process of thought, of cerebration. The original construction of the nest was instinctive or ganglionic, but the subsequent act was certainly cerebral; whilst both were perhaps of a decidedly "*intellectual character.*" Dr. Darwin tells the following anecdote:—"A wasp on a gravel walk had caught a fly nearly as large as itself. Kneeling upon the ground I observed him separate the tail and head from the body part, to which the wings were attached. He then took the body part in his paws, and rose about two feet from the ground with it; but a gentle breeze wafting the wings of the fly, turned him round in the air, and he settled again with his prey on the gravel. I then distinctly observed him cut off with his mouth, first one of the wings, and then the other, after which he flew away with it unmolested by the wind." Now, I take it that the mere pursuit and selection of the fly by the wasp as its prey was an act purely *instinctive*, whilst the clipping off of the wings under the circumstance narrated shows *it* to have been cerebral.

The constructive habits of the bee, as shown in the mathematical accuracy with which each cell of the honeycomb is formed, are certainly ganglionic or instinctive in their nature and origin; if the same structure had even been conceived and executed by man, it would have been an act of the brain, and an equal perfection of it could only have resulted from habit and experience. Similar observations will apply to the beaver, and not less so to many birds, &c. Such must be directly seen to be the legitimate and only conclusion, from the fact, that in some animals, without even a vestige of brain or spinal marrow, analogous phenomena are presented to our attention; the ganglionic system in them, as in the amy-encephalous monster of Dr. M. Hall, is the only power of any kind they can command; they possess none other, and therefore must it be conceded, that the "sensorial operations," so called, are occasionally performed independently of a brain; and what is more, "the actions which are at one time instinctive, may at another spring from a different principle."

Before proceeding to consider in detail the physiology of the solar ganglion and its dependencies, it is perhaps desirable to offer a few observations on the anatomical relations and dependencies of the three nervous centres. These will, it is hoped, facilitate the right comprehension of those physiological remarks which follow.

The brain, the spinal cord, and the solar ganglion, regarded respectively as the centres of the several nervous systems of man and the higher animals, though executing distinct functions in the economy of life, are, by means of the innumerable ramifications of their subordinate parts, and their union each with the other, brought into so close a connexion, that their physical separation is a matter of impossibility. Though not *mutually dependent*, they nevertheless exercise such a reciprocity of action, that the health and well-being of man is to regarded as inseparable from their union. The solar ganglion, brain, and spinal cord, may be together, not inaptly, compared to an ordinary tree, the root, trunk, and extremities of which may be said to severally represent them ; for the first has been the source, and continues to be the life of the other two; it has existed without them, and might again do so, though during their integrity it fails not to be the recipient of their toil, so to speak. The *root* will sometimes live on without the trunk and its extremities, and so will the paralyzed idiot. By referring to an engraving of the principal part of the organic nervous system, or sympathetic nerve, the anatomical connexions of the ganglionic with the spinal nerves, throughout the entire length of the cord, will be distinctly made out. It will be seen that each of the several ganglia, whether cervical, dorsal, or lumbar, gives off communicating branches with the individual spinal nerves, thus uniting the two systems. The solar plexus, the centre of the organic nervous system, is therein demonstrated as formed by the union of the two semilunar ganglia. It is described by Mr. Erasmus Wilson as "*a gangliform circle,*" from which branches pass off in all directions, like rays from a centre. It gives off distinct filaments, which accompany, under the name of plexuses, all the branches given off by the abdominal aorta. Thus we have derived from the solar plexus the—

Phrenic plexuses.
Gastric plexus.
Hepatic plexus.
Splenic plexus.
Supra-renal plexuses.
Renal plexuses.
Superior mesenteric plexus.
Spermatic plexus.
Inferior mesenteric plexus.

However, the principal nerve of the solar plexus is the great splanchnic. It arises from the upper and back part of the organ, and proceeding upwards, pierces the diaphragm immediately to the outer side of each crus, ascends in front of the vertebral column, within the posterior mediastinum, and terminates by dividing into five branches, which severally proceed to the sixth, seventh, eighth, ninth, and tenth dorsal ganglia. The pelvic viscera and the lower extremities are supplied with their organic nerves from the hypogastric plexus and sacral ganglia. The union of the ganglionic and spinal nerves would, in itself, be sufficient to establish a connexion between the former and the cerebrum and its nerves. They have, however, a more direct communication, by means of the ascending or carotid branch of the superior cervical ganglion, which, having entered the carotid canal with the internal carotid artery, divides into two branches, which form several loops of communication with each other around the artery, which constitutes the *carotid plexus*. Now the carotid plexus is the centre of communication between all the cranial ganglia; and, being derived from the superior cervical ganglion, between the cranial ganglia and those of the trunk: it also communicates with the greater part of the cerebral nerves, and distributes filaments with each of the branches of the internal carotid, which accompany them in all their ramifications. As each of the cranial ganglia, moreover, communicates freely with the cerebral nerves distributed about the head and face, exteriorly and interiorly, it follows that nothing more is required to establish the anatomical position which has been taken. I may add, nevertheless, that the pneumogastric nerve or vagus, by being distributed to the respiratory and digestive apparatus and heart, where it forms communications with the organic or vital nerves, as well as with the solar ganglion itself, by means of a small branch, which is represented in plate 19 of Mr. Wilson's work, before quoted, as being given off by the principal trunk, where it turns forward to be lost on the anterior surface of the stomach, yet more effectually secures the relationship between the cerebrum and the solar ganglion, the centres of their respective systems. The phrenic nerve, also, in being formed by the union of filaments from both the cervical and and sympathetic nerves, and in being further distributed to the diaphragm, provides for the same end.

The anatomical relations of the several nervous centres, as above explained, Mayo strangely regards as a reason why the "sympathetic nerves in human beings can only be regarded as branches of the cerebral and spinal nerves, destined for particular uses.

I have before treated of the "*nisus formativus*," of its nature and uses, as well as of its general independence of the brain and spinal cord, and I have therefore, to some extent, anticipated the observations I had to offer "*on the physiology of the solar ganglion.*" The reader will, I hope, excuse me if I recapitulate in a general way the line of argument there found.

In the first place, as regards the acephalous and amyencephalous monstrosities of Lawrence and Hall, we have the most conclusive evidence of the independence of the organic or ganglionic system of nerves on the cerebro-spinal system. We have a convincing and irrefragable assurance that the function of the solar ganglion and its dependencies is in itself as completely performed without the brain and spinal cord as with them. Who in looking to the acephalous, the brainless monster reported by Mr. Lawrence, would venture to declare that the influence of a *brain* was necessary to *life;* that *it* was anything more than an instrument *of* life. And who, in looking to the amyencephalous, to the brainless and marrowless monster described by Hall, could dare affirm that either the brain or the spinal cord were in themselves indispensable to existence; that is, to *life.* In both instances we are shown facts, which demonstrate to us as plainly and conclusively as can be demonstrated, the perfect and normal operation of the vital principle. We see the body nourished, the viscera perfected, and the bony fabric matured; and we are assured, therefore, that circulation, absorption, and nutrition, have been in full operation; in perfect action, without either a brain or spinal cord. It is truly absurd for Müller or any other physiologist to declare that "the *anatomy* of these monsters is *not* at present known with sufficient accuracy for any '*conclusion*' to be drawn from it."

The same cause which enables the abdominal and thoracic viscera of brainless and marrowless monstrosities to continue their functions, the identical source from which the heart and its vessels receive their power to circulate the blood; the liver, to secrete its bile; the stomach, to digest its contents; the intestinal absorbents, to take up the chyle; and the kidneys, to secrete their peculiar fluid, also empowers the organism of the decapitated animal in which the spinal cord has been also destroyed, to continue its specific action; the "*ultimate cause*" of the several phenomena of *life* is in both instances the same. "Clift saw the heart of a carp continue to beat eleven hours after the destruction of the spinal cord;" and Müller tells us that "in fishes the contractions of the heart continue for the space of half a day after the destruction of the brain and spinal marrow." The same author obstinately adds, "that the brain and spinal marrow must nevertheless be regarded as the principal source of the nervous influence; the cardiac nerves, under such circumstances, still retain a portion of the motor influence." In the instance of the frog experimented on by Redi, which lived "six complete months" after the "removal of the whole brain," as well as that of the animal operated on by Sir B. Brodie, in which the posterior part of the spinal marrow was removed, without affecting in any degree the strictly *vital* actions natural to those parts which receive their nerves from it, would Müller venture to assert that, in either case, the phenomena observed were referrible to the circumstance of the retention of a certain power or principle of action, originally derived from either the brain or the lost portion of the spinal cord? Impossible! Nothing is more certain than that the destruction or *removal* of the solar ganglion and its dependencies is immediately fatal to the functions of the brain and spinal cord, no less than to those of the other corporeal organs, as the liver, stomach, &c.; and it is equally true that the destruction or removal of either the brain or spinal cord, or even of both of them, *if effected with care,* may be accomplished without impairing the energy of the ganglionic system. If you destroy the brain and spinal marrow of a frog, you will not *kill* the animal; the heart will still continue to beat, and the digestive process be carried on. A slight blow on the epigastrium, *over the solar ganglion,* will sometimes kill a man, but extensive injuries of the brain and spinal marrow are not unfrequently borne with comparative impunity. Pressure on the solar ganglion of dog or cat or rabbit, will quickly destroy life; and if it be carefully made over the upper part of the *lumbar region,* where the experimenter will avoid the solar ganglion, the lower extremities will become completely paralysed: the animal will merely draw them after him, as if they were but artificial appendages. Remit the pressure, and in a few minutes the spinal functions are restored, and the animal is as lively as ever. This, in itself, is a beautiful illustration of the dependence of the vitality of the lumbar spinal cord on the contiguous ganglia of the sympathetic. In the lowest classes of animals, we see the existence of the same nervous organism without the addition of parts which characterize those higher in the scale of being; in them there is observed a ganglionic system only, and therefore it is that they execute functions purely preservative. We may ask, if a ganglionic system be deemed sufficient for the vital actions of one class of beings, why should it not for another? Digestion, circulation, secretion, absorption, &c., are no more perfectly performed in man than in the oyster or medusa. *Life*, then, is identical throughout animate nature, and if a particular nervous power be held sufficient in one instance for the effects observed, why should it not in another, or even in all? There is this difference between the lower and more perfect animals; that whilst in the latter the vital principle is, it would appear, generated in a central organ, and from it diffused through the organic nervous system, in the former it is, I apprehend, produced equally by all the ganglia of the sympathetic; and therefore it is that, "the division of these animals into separate fragments does not destroy the organism, but, on the contrary, gives rise to the production of several distinct beings." It would seem that the addition of a spinal system to a ganglionic, as in the annelidæ, constitutes the limit within which nature allows the divisibility of the vital principle. "The articulatæ, although, like the annelidæ, they have a ganglionated nervous cord, do not continue to live when thus divided." The first possesses, in addition to ganglionic and spinal nerves, a medulla oblongata, from which nervous branches arise which furnish the animal with the use of its external senses; and these establish so indissoluble a connexion, and so direct a dependence on the external world, that the facilities which the animal enjoys more than compensate for the loss of the other property. A step, is, moreover, made towards that more perfect organism wherein the increase in number and complexity of its parts tend, in an eminent degree, both to establish the responsibility of man, and to explain the nature of those physical laws to which *man* must submit, if he would avoid the many miseries which now press on him from all sides.

We may, with much propriety and truth, regard the permanent condition of the medusa, for instance, as characteristic of the temporary, or accidental, or acquired condition of *man.* In the very early period of fœtal life, we observe nothing more than a gelatinous, shapeless mass, in which the microscope can nevertheless discover a nervous ganglion or ganglia. *Life,* in both, is complete. This is man's temporary condition; the same is permanent in the polype. In the amyencephalous monster—that is, the brainless and marrowless fœtus—we see the ganglionic life of the medusa accidentally occurring; and similarly so in the animal which has been deprived of its brain and spinal marrow; as well as in the individual under the influence of animal magnetism; when, as oftentimes happens, the cerebral and spinal functions are completely obliterated, and the patient, of course lost to all consciousness and feeling, realizes the precise condition of one in a *profound* sleep.

From the observations already made, both in this and the preceding chapter, it is seen that the nutrition, growth, and reproduction, no less than the decay, of the organism, must be attributed to the operations of the organic nervous system. That the nutrition of all parts of the body, in conformity with the original type, presupposes the persistence of that power which originally produced all the distinct parts, all the organs, as "members of one whole," as parts necessary to our idea of the being; and which is present in the germ before any distinct organs are formed, while the animal exists merely "potentially."

Nutrition, then, is the continued reproduction, as it were, of all parts of the animal by this internal power; but in the adult, the reproduction can be effected only by the process of assimilation—that is, by the union of new matter with the assimilating parts; while in the embryo, in which no organized "groundwork" as yet exists, the parts are formed, their "groundwork" is, in fact created, by the *formative power,* which is still undivided. "For although, until the whole body perishes, all the organs are directed by *one formative principle,* which produces the concurrent action of all assimilating tissues, and the operation of which we admire, as the vis medicatrix naturæ, in the correction of the subtile material changes which are induced by disease; yet organized parts of the body, once formed, cannot, in most cases, if wholly destroyed, be again restored by this power, this vital organizing principle," (Müller,)—which emanates from the solar ganglion, the central organ of the ganglionic nervous system.

The observations made by Müller in treating of the influence of the nerves on nutrition, are singularly contradictory. His argument is to the effect, that although the nutrition of monsters "is by no means defective," but their development up to the period of birth, even *perfect*, yet "is there no proof that the sympathetic nerve has a more especial influence on *nutrition* than the cerebro-spinal nerves, except, perhaps, the fact that the nutrition of a part does *not* cease when the nerves which it receives from the brain or spinal cord are divided"—a fact, which *I* must ever regard as a sure indication that the organic nerves can be none other than the nutrient nerves. Such is the inevitable conclusion. Why, it may be asked, does an injury or disease of the superior cervical ganglion arrest the nutritive processes in the eye. The branches of the third, fourth, fifth, and sixth pairs of nerves, usually distributed to the contents of the orbit, are untouched, and nevertheless, the eye loses its vivacity, and becomes *disorganized*. We have, then, in these instances, as in many more, both *positive* and *negative* evidence of the truth of my position—viz , that the solar ganglion is the SEAT OF LIFE; and that by means of its dependencies the vital principle is diffused through the organism. Further, in order to show that the organic nerves do not influence nutrition, he, Müller, says, "Where any organ is wanting, there is always a corresponding absence of their *nerves*," meaning their cerebro-spinal nerves, "absence of the eyes is attended with absence of their nerves." This is a mere assumption, and to prove it so, I beg to offer for the consideration of the reader the following *extracts* from a report of my own to be found at page 293 of vol. ii. of 'The Lancet' for 1835 and 1836, concerning the post-mortem examination of a child born without eyes: "On raising the anterior lobes of the cerebrum, but *one* olfactory nerve was seen, and that was on the left side; the optic commissure was placed more posteriorly than natural; the sella turcica being much anterior to it. From its forepart, and in the median line, the rudiment of a nerve was continued, it was of a loose texture, and of a scarlet hue, as if from injection of its vessels. It proceeded into the substance of the pituitary body, in which it was lost. Several small bloodvessels passed through the foramen opticum; the fissura lacera transmitted both into and out of the orbit, the ordinary nerves and the ophthalmic vein." "The orbital roofs were then removed, and disclosed the presence only of condensed cellular tissue, which, when cautiously dissected away, exposed the several small muscles which naturally belong to the organ of vision, supplied with their respective nerves."

Sir Benjamin Brodie truly says that the processes of nutrition, growth, &c., must be referred to the same class of functions as that of secretion generally. The following experiments prove, not only so much, but also the relative properties of the cerebro-spinal and ganglionic nerves. In a young cat, the terminations of the nerves of the eighth pair on the cardia of the stomach were carefully divided; the animal was perfectly well afterwards; was lively, ate its food as usual, and the respiration was not affected. At the end of a week, and three hours after having been fed with meat, the cat was killed. On dissection, digestion was found to be going on as usual; the food in the stomach was in a great measure dissolved; and the thoracic duct and the lacteals were distended with chyle, having the ordinary appearance. The nerves were carefully traced, and it was ascertained that not the smallest filament had been left undivided. This experiment was repeated with exactly the same results. It was long since asserted, "that the nerves of the par vagum are by no means necessary to digestion;" and more recent experimenters, including Magendie and Dr. J. Reid, have arrived at a similar conclusion, provided their division is made in such a manner as not to affect respiration and the free oxygenation of the blood.

A portion of the anterior and posterior crural and sciatic nerves was removed in a dog in the upper part of the thigh: the limb, of course, became immediately benumbed and paralyzed. A wound was then made in the leg, and the claws of all the feet were cut so as to make them of the same length. The wounds suppurated and healed as readily as if the nerves had been entire. At the end of seven weeks I measured the claws of the four feet, and found that those on the paralyzed limb had grown equally with those of the others.

The experiment was repeated by Sir B. Brodie on guinea pigs, and with the same results. In one of them the tibia was broken, and the bone readily united.

The posteror part of the spinal marrow of a frog was removed so as to destroy the origin of the nerves of the lower extremities. In this case the bones of one leg were broken, and when the animal was killed the bones were found united, partly by callus and partly by bony substance. Experiments, very similar in their nature and effects, have been performed by Dr. Sharpey; and Müller says, by Schroeder. I have also practically verified the position here taken—viz., that nutrition, growth, and reproduction, are equally the functions of the organic nervous system, both by experiments on living animals and by observing the recovery of the demented and paralyzed among the insane, from various kinds of bodily injuries (including fractures) and disease.

Whilst, however, Müller contends for the reciprocal dependence of the organs, and of the cerebo-spinal nerves, on each other for their existence he cautions his readers from concluding that the production of the former depends upon the pre-existence of the latter. He truly says, "both organs and nerves are produced by the same power, the *nisus formativus*, which resides undivided in the germ."—*Lancet, Sept.* 14, *p.* 314; *Sept.* 21, *p.* 339; *and Oct.* 26, 1850, *p.* 471.

15.—ON THE TREATMENT OF DELIRIUM AND COMA.

By Dr. R. B. Todd, F.R.S.

[We have already, in 'Retrospect,' Vol. XXI., referred to Dr. Todd's admirable Lectures on this subject before the College of Physicians. He dwells again on this interesting subject in his course of Lectures at King's College, with perhaps more particularity, and we are sure that we cannot too often bring his views before our professional friends. Dr. Todd commences by saying that from the great extent of the subject, he was unable to enter at length on the treatment of these diseases at the College of Physicians, and therefore proposes to discuss it more particularly under a separate head. Before speaking of the treatment, however, a few words with regard to the *diagnosis* are necessary, not only of each form of delirium or coma from the others, but especially of the inflammatory from the non-inflammatory forms of delirium and coma. Dr. Todd says:]

In this latter diagnosis, it is plain that we derive great aid from the clinical fact to which I have already alluded; namely, that the vast majority of cases of delirium and coma are clearly not dependent upon any inflammatory process in the brain—upon any process which leads to the formation of lymph or pus, or to the ultimate disorganization or total destruction of the tissues affected.

And we derive further help, in the diagnosis, from the fact (which, I think, cannot be doubted), that inflammation of the brain (whether of the membranes or of the cerebral pulp) is a rare disease, excepting in quite young children. It is seldom met with, both in hospital and in private practice, excepting at those early periods of life when it is associated with a strumous state of constitution.

Hence, there being the strongest reasons for concluding that inflammation of the brain is a rare disease, we are justified in holding the opinion that delirium and coma, in the vast majority of instances, are non-inflammatory affections, but that occasionally they depend upon, or are associated with, inflammation of the brain.

What, now, are the symptoms which, in any given case, would lead us to affirm that the patient is suffering from inflammation of the brain? I shall enumerate these symptoms in the order of their frequency and importance.

The first symptom which I shall mention, because it is of very frequent occurrence, and ought to excite very strong apprehensions in the mind of the medical attendant, is *nausea* and *vomiting*. It comes without any assignable cause; sometimes the patient vomits without any previous warning, and quite irrespective of any food he may have taken into his stomach, or of any previous derangement of his digestive organs, or, indeed, of any previous marked disturbance of the general health.

Secondly, we meet with pain in the head as a very frequent symptom, which, however, exhibits a good deal of variety in its characters, Sometimes it is acute, and fixed in its situation; at other times dull and indefinite. It is more intense and better marked in proportion as the disease is seated nearer the surface. It is most intense in inflammation of the dura mater, less so in inflammation of the pia mater, and least severe in inflammation of the cerebral pulp. When the inflammation arises from a syphilitic taint, the dura mater is very frequently affected; and in such cases the pain in the head is not uncommonly accompanied by a tenderness of the pericranium in the position of the pain.

Thirdly, sluggishness of the pulse is a frequent accompaniment of even the most intense inflammation of the brain. The pulse may fall as low as 50, or even 40, or it may continue to beat at the rate of 60 or 70. It may be sluggish without being particularly slow. The artery strikes in a heavy sluggish manner, not rapidly or sharply, but conveying the sensation as if it impressed a larger surface of the finger than usual. Generally, however, in cases of inflammation of the brain, the pulse is both slow and sluggish, and the heart's action heavy, the beats not succeeding each other rapidly.

Fourthly, we have delirium tending to coma. In inflammation of the brain the delirium is generally of the low and muttering kind. You seldom or never have wild frantic delirium when there is inflammation of the brain. The descriptions given by systematic authors of what is called *phrenitis,* appear to me to have no foundation in clinical study—at least, if that affection be regarded as a form of inflammation within the cranium. In inflammation of the brain there is, along with the delirium, a more or less comatose state, which gives to this form of delirium a close resemblance to that which comes on in typhus fever. Indeed, some cases of inflammation of the brain so much resemble typhus fever, that it is exceedingly difficult, or quite impossible, to distinguish between them, until the disease is so far advanced as to afford some special characteristic symptoms.

Fifthly, I may notice *coma,* which generally accompanies or follows delirium, and which is a more constant symptom of inflammation of the brain than delirium. Delirium of the low muttering kind, passing readily into coma, without any evidence of diseased kidney, or of impediment to the excretion of any other gland, affords strong presumptive evidence of the existence of inflammation of the brain.

The last symptom to which I shall refer as accompanying inflammation of the brain is *paralysis,* which occurs under a great variety of circumstances as regards degree and locality. Occurring in connection with the other symptoms I have detailed, it is a sure indication of the presence of destructive disease within the cranium. It is obvious, however, that it must vary considerably in extent, according to the extent and position of the inflammation. You may have a slight paralysis affecting the levator palpebræ and some of the muscles of the eye-ball; or a hemiplegia, perfect or otherwise, but frequently exhibiting a more or less contracted state of the muscles of the paralytic limbs.

The paralysed limbs are sometimes effected at intervals with epileptiform convulsions, more especially when the inflammatory process is seated on, or very near, the surface of the brain. In rare instances, in young subjects, such partial convulsive movements may pass into general convulsions: otherwise, I apprehend that general convulsions cannot be regarded as a feature of the clinical history of inflammation of the brain.

Thus, then, that form of delirium which is accompanied with nausea or vomiting, pain in the head, or a slow sluggish pulse, and has an obvious tendency to the comatose state, or passes quickly into coma, and especially when accompanied or succeeded by paralysis, may be justly viewed as having its origin in inflammation within the cranium, more especially when there is no evidence of disease of the kidney.

Excluding the delirium which originates in inflammation of the brain, the diagnosis of the other forms of delirium from each other is comparatively simple, certain salient points in the history of each affording generally obvious indications of the nature of the delirium.

[Dr. Todd considers the treatment of *delirium tremens* first, not only because we see it most frequently in hospital practice, but because the treatment best suited to it is also in a great degree applicable to other forms of delirium. He says:]

By delirium tremens I mean that state which comes on sooner or later in all persons addicted to habits of intemperance. Many practical men recognise two kinds of delirium tremens; but it would, as it seems to me, be more correct to consider these as two distinct forms of delirium, closely allied, however, inasmuch as they both may arise from intemperance.

The first is the true delirium tremens of drunkards,—*delirium ebriosorum*; the second is the delirium from alcohol,—the *delirium e potu*, which may occur in a man who is habitually the most temperate. As I go on you will see the importance of this distinction.

The one occurs in a habitual drunkard,—the other may occur in a person drinking for the first time. One is frequently the precursor of the other.

If a temperate man be led to drink excessively, the second form soon comes on, arising from the poison of alcohol interfering with the healthy action of the brain; but if he become a habitual drunkard, he is subject to attacks of the true delirium tremens, under particular circumstances. In this case, however, the poison is not alcohol, but a compound formed of alcohol and perhaps some morbid matter generated in the system.

The true delirium tremens is the delirium of a man who, having long indulged in his cups, has by some means or other been depressed, or prevented from taking his accustomed amount of stimuli.

The delirium e potu is the delirium of a man whose blood has become charged with alcohol: it is a case of active poisoning, the poison being alcohol: but we know this cannot be the case in the delirium of drunkards, because the exhibition of alcohol in some form or other is known by experience to constitute a most important part of the treatment of the disease; as if alcohol, within certain limits, were an antidote to the poison which disturbs the nutrition of the brain.

If these distinctions be correct, it is obviously of vast importance to recognise clearly the two maladies, as an essentially different treatment would be required. In the one case you have to deal with delirium supervening upon a disease in which the powers of life have been greatly impaired by a course of slow poisoning: in the other you have to oppose by your treatment the mischief done by the introduction of alcohol, and to eliminate this poison from the system.

You have very much the same difference in cases of opium-poisoning as in that of poisoning by alcohol. Habitual opium-eaters are liable to a form of delirium which closely resembles the true delirium tremens, and which, like it, is apt to come on when they are deprived of their accustomed supply of opium. But the rapid administration of opium in large quantity will excite a state of delirium which very speedily passes into profound coma. In the one case, the administration of a certain quantity of opium, or of some other sedative, is essential to the cure; in the other case, the introduction of any more opium into the system would inevitably destroy the patient, or greatly increase his peril.

It is important to observe that these two kinds of delirium may be mixed: they may occur in the same patient at the same time. A man habitually intemperate, whose blood is already poisoned by the peculiar poison of delirium tremens, and who has had the horrors, and many other of the symptoms of delirium tremens, sits down to a bout of drinking, and alcohol enters his blood in large quantity. Thus he becomes at once the subject of two states, of which, however, the delirium e potu would evidently predominate. In such cases the treatment evidently becomes much complicated, and it would obviously be desirable to eliminate the free alcohol from the system before you can deal with the delirium tremens.

I shall now proceed to the consideration of the treatment of the true delirium tremens, which we meet with very commonly in hospital practice, both in a mild and in a more intense form.

I. The milder form of the disease is characterised by the occurrence of horrors, hallucinations, tremor, vigilia. In such cases the patient fancies he sees demons, or dragons, or insects creeping over his bed, or flying in the air; he is suspicious, and thinks everybody is combining against him to do him some injury; or he dreads some impending evil, and fancies himself about to die, or to fall into misfortune. With some or all of these symptoms there is more or less of tremor accompanying the voluntary motions. If you ask the patient to hold out his hand, he does so with a remarkable and characteristic tremor: this tremor interferes greatly with the due performance of many acts which require a nice muscular adjustment: he cannot hold and direct his pen properly: he is a long time about buttoning his coat or his breeches, or tying his cravat: to pick up a pin, or neatly insert it into any part of his garments, is quite beyond his power. So also if you make him shut his eyes strongly, or protrude his tongue, you witness the same characteristic tremulousness in those actions. He finds it difficult to obtain sleep, and if he does sleep it is only for short periods, frequently being waked up in a fright by horrible dreams, or with nightmare. With all these symptoms of disturbance of the nervous centres, the patient's skin is moist, often perspiring freely, his tongue is clean, and his pulse soft, and not quick.

In every case of this kind it is important to remember that the balance of nutrition, so to speak, is disturbed by some cause,—either by the want of proper food, from loss of appetite or from the inability to procure it, as we often find with our poor hospital patients; or by the impairment of the functions of the stomach, through the repeated use of stimulants, so that the patient is unable to retain or digest nutritious food.

The great object of treatment, therefore, is, to restore the balance of nutrition and to procure sleep, in which, if you succeed, you cure your patient in a very few days. It generally happens in these mild cases that the discipline, the regular hours, and the care to which the patients are subjected in a hospital, restore them to health with very little medical interference.

[Dr. Todd illustrates the preceding observations by the particulars of the following case:

Henry Arden, aged 29, was admitted on the 2nd of July. His health had been good until nine months previously, when he suffered from some indisposition. He had acquired the habit of drinking porter daily, even to the amount of from four to five pints, from which he supposed he derived benefit.

Two months before his admission, he began to suffer from dyspeptic symptoms, with loss of appetite. He left off taking his porter, and, in his own words, "felt something rise from his chest to his head," accompanied by various hallucinations, (fancying himself to be walking about the house, opening windows, &c.), and these continuing, he was admitted on the 2nd of July. On admission, he exhibited all the characteristic signs of delirium tremens, tremors in all his movements, pallid countenance, sleeplessness, with the above hallucinations.

He was put simply upon a regulated diet, allowed a pint of porter daily, and a moderate quantity of meat, and ordered half a grain of morphia every night. On this night, he slept for the first time for four nights. On the fifth, the tremulousness was less, and all hallucinations had disappeared; and on the 12th, with the exception of some tremulousness remaining, he was quite well.

Dr. Todd then proceeds to consider the treatment of the more intense or confirmed delirium tremens.]

In deciding upon the appropriate treatment of such cases as these, we must look in the first instance to the immediate cause which has disturbed the balance of nutrition. We shall find this to be either that he has been deprived of food by dire want, or that his stomach has become so irritable that it rejects everything that he takes. I say it is important to determine the cause of this disturbed nutrition; because our first endeavour must be to induce our patient to take nourishing food. This will be easily enough effected if the patient had been deprived of food from necessity, or if his appetite had been impaired; and it is best to administer the food in small quantities at a time, but frequently repeated. An ounce, or an ounce and a half, of mutton-chop may be given every two or three hours; or small quantities of some good animal broth or soup. The most difficult case to deal with is where the stomach has become extremely irritable, and rejects the food. Under such circumstances, the greatest nicety is required in the administration of nutriment. If the stomach will not bear solid food or animal broths, milk may be given, the caseine of which may serve for solid food; but it must be given in small quantities at a time, and it may be combined with lime-water, or sometimes even with very small quantities of some alcoholic stimulant—brandy, rum, and gin, it being generally better to select that stimulant which the patient has been accustomed to drink. The patient may be able to retain nourishment of this kind if given in tablespoonfuls or even teaspoonfuls at a time, while, if larger quantities were administered, vomiting would ensue.

At the same time, we may with advantage attempt to allay the irritability of the stomach, by other means likewise—as by giving ammonia in effervescence, or the common effervescing draught of soda or potash, or by prussic acid given with or without effervescing medicines; or we may have recourse to a very useful and effective remedy—kreosote combined with morphia, in small doses in the form of pills. Iced water, or small quantities of the pure Wenham Lake ice, will allay the irritable stomach when other means fail. Counter-irritation over the region of the stomach may be also employed with benefit—as mustard poultices, or flannels wrung out of hot water and sprinkled with turpentine.

The mucous membrane of the stomach is no doubt in these cases sometimes in a state of actual inflammation, or nearly approaching to it, although less frequently so than many suppose; and it may be desirable to apply leeches to the epigastrium; but as a general rule, it behoves us to be cautious about taking blood in these cases.

I need scarcely add, that in all cases where the stomach is highly irritable it is very important to have the bowels freely evacuated, either by purgatives or by enema.

Let us now suppose that your patient, not having slept for many hours, is in a state of delirium so violent that he cannot be kept in bed, and is with difficulty kept from injuring himself or others; how are we to act in such a case?

There are three points to which your attention must be especially directed:—

1. The due administration of nourishment.
2. The means of restraint.
3. How to procure sleep.

I have already sufficiently considered the first point, and shall only add, that whatever the nature of the delirium, this point is primary and paramount.

[With regard to the second question of restraint, Dr. Todd says, although the patient should be prevented injuring himself and others, yet the straight waistcoat is to be avoided in every case if possible; although in one sense secure, it is a bad practice, and as you likewise want to procure sleep and to preserve strength, its use is almost incompatible with these objects. The main objection to its use is that the patient is continually using the most forcible efforts to extricate himself from it. His mind is excited with the idea that he is held down by people sent to torment him, and he uses all his might to free himself from them; and hence it is Dr. Todd dwells upon the restraint being made chiefly by watching,—though there are some instances in which the straight waistcoat may be absolutely required. As to the third point, Dr. Todd remarks, there is no remedy to procure sleep better than crude opium or laudanum; and as these are known to contain all the active principles of the opium, the chances of obtaining the sedative and narcotic influence are greater with it than with morphia. Dr. Todd continues—]

When you give opium, do not be timid with it. Give a full dose at once, two or three grains, or a drachm of the tincture, and be guided by the progress of the case as to the frequency of its repetition and the amount of the subsequent doses.

The combination of camphor with opium frequently counteracts the unfavourable, and promotes the sedative influence of the latter. But I know no combination which, generally speaking, answers better in delirium tremens, especially where there is much depression, than that of quinine with opium. One or two grains of quinine, with from ten to thirty drops of laudanum, given every four, six, or eight hours, often answer extremely well.

Many practitioners, and amongst them some whose opinion is entitled to the greatest respect, have recommended a combination of tartar emetic and opium, and place great faith in this admixture; but I think a fallacy may exist here, in the fact that opium undoubtedly modifies and controls to a great extent the action of tartar emetic, so that in fact, whilst administering the two drugs, we may in truth be simply giving opium. If you could always succeed in limiting the influence of the tartar emetic to its simply alterative effects, there would be no objection to its use, as its action on the skin would materially assist in eliminating poisonous matters from the system, but its depressing influence does positive harm, and is not always so readily counteracted as is desirable. For this reason I seldom or never have recourse to this combination.

Another expedient which you should keep in view, as a means of quieting delirious patients, is the application of cold to the head, either by a bladder of ice or by the use of the cold douche. Both, you must remember, are powerful agents, and tend to produce depression of the heart's action, which may be carried to such an extent as to kill the patient, if the douche be applied too powerfully, or the ice be kept applied too long.

Sometimes, when we have persevered with opium for some time, the pupil becomes contracted, and yet the patient does not sleep. In such a case are we to persist in giving opium? I think a very contracted state of the pupils ought always to be taken by the practitioner as a strong indication against the further use of opium, and that then he may have recourse to other sedatives. Under such circumstances I incline to think we have a most valuable resource in the administration of chloroform by inhalation, but I have not as yet had sufficient experience of its effects to justify me in speaking very positively respecting it. I shall relate to you three cases, in which its use was followed by the most happy results.

The first case was that of a man in the hospital, who had been two days under treatment, and had taken plenty of nourishment and opium, and the pupils were so contracted that we were afraid to persist in the use of the latter. I therefore determined to try the inhalation of chloroform, and had it administered while I was in the ward, taking care to feel the pulse during its inhalation. The man was soon under its influence, and slept continuously for twenty-two hours, only waking once during this time to take nourishment. The report in the case-book states that this was the first sleep the patient had had for five days. He soon got quite well.

The second case likewise occurred in the hospital: opium, although not tried so long as in the first case, failed to produce sleep: chloroform was administered; the patient slept all that night, and the next morning was perfectly quiet, and soon went out restored in health.

A third case occurred in private practice, in a patient whom I saw with Mr. Simon. He had been treated in the most careful and judicious manner previously to my seeing him, and opium had been freely given without causing sleep, whilst it produced contracted pupils. It was now a question as to the propriety of administering chloroform, and I had no difficulty in agreeing with Mr. Simon to give it. It was accordingly done, although to effect it we had to use force: the patient slept soundly after it, and recovered very speedily.

[Dr. Todd now impresses certain cautions which it is very necessary to observe with reference to the use of chloroform. These are,]

1. That during its administration you must always keep your patient in the horizontal posture.

2. That, whilst the patient is inhaling, you should watch carefully the state of the pulse; and, if you should observe any marked alteration of an unfavourable kind in its strength or frequency, you should instantly desist from administering it.

3. That you should always examine the heart previously to the inhalation; and, if there be any decided disease of that organ, you should not administer it at all. Chloroform is a very depressing agent, and it should therefore not be used without great carefulness on the part of the practitioner.

I may also add, that it is important, when possible, not to administer chloroform to a patient shortly after he has taken food, as under such circumstances the chloroform is very apt to excite an emetic effect, which would greatly embarrass the practitioner in his course of treatment.

There is a symptom, which patients affected with delirium tremens sometimes exhibit, much more difficult to combat than sleeplessness—namely, the obstinate refusal to take food. This is a most unfavourable symptom: and when it is present in a very marked manner, the prospect, both for the patient and practitioner, is most gloomy, because that first and most important indication in the treatment of the case—namely, the restoration of the balance of nutrition, cannot be fairly fulfilled.

In such a case we must devise some means of conveying nourishment into the system, either by persuasion or by force. One good plan is to give enemata of some highly nutritious broth, such as strong beef-tea. I frequently order a quarter, or half, or even a pint, of strong beef-tea, with ten or fifteen grains of quinine, regulating the quantity of the liquid by the probable facility with which the patient would retain the enema. Food may also be administered by the stomach-pump and œsophagus tube. For both these methods of giving nourishment force must be employed: less force will be required for the former than the latter, and therefore it is well to give a trial to the enema plan first, It will not do alone for any length of time, and therefore you will be obliged to have recourse to the œsophagus tube. The objection to the use of the stomach-pump and the œsophagus tube is, that in some cases great resistance is offered by the patient, and his struggles are apt to cause great exhaustion: and more than once I have seen unfavourable results follow this treatment; but in many cases you have no alternative.

In a few cases of this kind I have made the patient inhale chloroform, and, while he was under its influence, given food by introducing a spoon carefully into the mouth to the back of the throat, so as to excite the muscles of deglutition. The food conveyed to the back of the pharynx, within the grasp of the posterior pillars of the palate, produces reflex action of the constrictors, and is immediately swallowed. You must be careful, in giving food in this way, not to give a large quantity at once, as you thereby run the risk of making the patient sick.

An important question, as regards the general treatment of delirium tremens, is as to the propriety of bleeding. Is it necessary or desirable to bleed, either generally or locally, in such cases? Upon this point practical men are pretty well agreed, that bleeding is a bad and highly dangerous practice; and both reason and experience combine in support of this opinion. For in this delirium there is a marked tendency to the deterioration of the blood by increase of its water and a diminution of its colouring matter: and we know that under a bleeding system this tendency becomes augmented, and so the disturbed nutrition of the brain increased and perpetuated. Moreover, there is nothing in the pathology of the disease which would warrant us in blood-letting; for we have it ascertained on the most incontestable evidence that there is no inflammation or congestion to combat by such means. Again, experience shows that bleeding is followed by unsatisfactory results. If you bleed in violent delirium, you generally add fuel to the fire, and you increase the violence of the delirium; unless, indeed, you take away so much blood as to produce extreme prostration, which destroys the force of the patient, and from which he is not likely to rally.

In fine, in the treatment of the ordinary cases of delirium tremens which you will meet with in practice, I cannot too strongly impress it upon you, that the most important point is to uphold the strength of the patient, in which, unless you can succeed, all other remedies will useless. In conformation of this, I may here mention, that Dr. Gerrardt, of Philadelphia, as I learn from Dr. Wood's excellent book on the practice of physic, pursues with great success a purely stimulating plan of treatment in delirium tremens, to which alone he trusts; he gives in all cases, an ounce or two ounces of brandy, every two, three, or four hours, according to circumstances; and by following this plan he has reduced the mortality from this disease in the large hospital at Philadelphia, from one in eight, to one in thirty-nine. This is a very important statistical fact in reference to the treatment of delirium tremens, and affords valuable support to the advice I have given you. I am not, however, prepared to trust to this treatment alone, nor to give up the use of opium and of other remedies.

In some cases of delirium tremens there is a decided tendency to a comatose state, which you must be prepared to deal with. In such cases, the rule to uphold the powers of the patient as much as possible applies equally with the others; but it is plain that you will have to give up the use of opium and all other sedatives, as tending to increase the coma.

Delirium e potu.—Let me now speak of the treatment of cases suffering from the *delirium e potu,* a state which, we have seen, is in many respects essentially different from that of the true delirium tremens. A patient is brought into the hospital in a state of violent delirium, talkative, perhaps singing or hollowing, and requiring considerable restraint to hold him down; and we find, on inquiring, that he has been drinking; and we have evidence of that in the odour of alcohol from his breath. The long continuance of this state leads to coma. Now in these cases, which are examples of direct poisoning by alcohol, the great object of the treatment is to promote the elimination of the poison without unduly depressing the vital powers. This may best be done by emetics, purgatives, and sudorifics. You would give emetics, if you had reason to believe that there was alcohol still in the stomach, or indigested food. Nauseating doses of tartar emetic are useful in reducing the force of the heart's action, and in promoting free diaphoresis. It must be in cases of this kind, and not in those of true delirium tremens, that croton oil has been, as I have heard, used with great success by some practitioners in the country; I have no experience of it myself. It may be that such a practice would tell well in the mixed cases.

A sudden splash upon the head, and face, and chest, with a large quantity of cold water often exercises a decidedly sedative influence; but if this fails, you need not have the same difficulty about using mechanical restraint in this as in the cases of true delirium tremens; and then you will find it useful to keep cold well applied to the head.

In these cases local bleeding may be sometimes practised with advantage; but it should always be tried with caution, and you should bear in mind that the poison of alcohol is apt, when given largely and quickly, to leave behind it a state of depression, which bleeding is apt to increase. In many of these cases there is a highly irritable state of stomach, due to a state of mucous membrane which is probably inflammatory, and brought on by the direct influence of the alcohol. When this is the case, of course you will not give such remedies as tartar emetic, but you will direct your treatment to the irritable state of the stomach. Counter-irritation by mustard to the epigastrium; ice or iced water in small and frequently repeated quantities; cold drinks; prussic acid,—will very generally succeed. If these means fail, then try leeching the epigastrium, morphia and creosote, effervescing draughts with potash, soda, or ammonia; and it is very important to keep up a free action of the bowels, which may best be done by small and often repeated doses of sulphate of magnesia, dissolved in water, which of themselves tend to relieve the irritable state of the stomach.

In cases where the true delirium tremens is complicated with the delirium e potu coming on after a debauch, we must endeavour to restore the balance of nutrition in order to cure the delirium tremens, and also apply the proper remedies to relieve the delirium e potu; we must, in fact, combine, as far as possible, the two forms of treatment.

[In all cases of delirium connected with pneumonia, Dr. Todd advises the stimulating plan of treatment, and in illustration of this subject, he relates two cases of pneumonia in which this symptom occurred, which were thus successfully treated; and although the delirium in the second case did not go beyond a slight wandering at night, yet he attributes the almost total absence of it to the fact that the stimulants were early and freely administered. Passing on to the delirium of bronchitis, Dr. Todd continues:]

The delirium of bronchitis should be dealt with in every way as the delirium of pneumonia. Depression of the vital powers favours the occurrence of delirium; and bronchitis is, generally speaking, a very depressing disease. If therefore, you should be attempting to cut short the inflammation in a case of bronchitis by active antiphlogistic treatment, you may look upon the occurrence of delirium as the signal for you to alter your treatment, and to adopt a stimulating plan. There is a great tendency in the delirium of bronchitis and of pneumonia to pass into coma; and, unless prompt means are adopted by the physician, the patient dies. This tendency to coma is certainly greater when antiphlogistic remedies have been used. When coma comes on, you must not relax in giving support; indeed, the failure of the pulse or of the heart's action may induce you to be more active in the administration of stimulants: but, in addition to this, you will probably find great benefit from free counter-irritation, by mustard or blister, or both, to the back of the neck; and it may be necessary to shave the head, and apply a blister to the scalp.

Sometimes but very rarely, the coma coming on suddenly in this way may arise from a sudden effusion of blood upon or into the brain.

Rheumatic and gouty delirium.—I shall now proceed to speak of the treatment of the rheumatic and gouty forms of delirium.

Rheumatic delirium generally comes on suddenly in the course of rheumatic fever, and is frequently *coincident* with pericarditis and endocarditis. I say it is *coincident* with the cardiac affection, and not necessarily related to it as effect to cause; because the amount of severity of the delirium bears no proportion to the intensity of the inflammation; for we may have slight peri- or endocarditis with severe delirium, or we may have slight delirium with extensive inflammation of the heart; and we know well that the cardiac inflammation often exists without the delirium. The severity of the delirium is, however, in proportion to the debility of the patient. When you have a very watery state of the blood, or where the blood is deficient in colouring matter, and when, also, perhaps, the blood is much charged with the rheumatic poison, you have a condition highly favourable to the production of delirium.

The rheumatic delirium undoubtedly occurs more frequently in those cases of rheumatic fever which have been treated by bleeding; and probably because bleeding makes the blood watery, and diminishes its red particles. When I was in the habit of bleeding in every case of rheumatic fever, as I used to do some years ago, a much larger portion of my patients were attacked with delirium than now, when I never bleed in this disease: indeed, now I find it difficult to meet with a case of this form of delirium to show you.

Now let us suppose that you are asked to see a patient who had been labouring under rheumatic fever, and who has suddenly become delirious, with or without difficulty of breathing. From what you now know of the clinical history of the disease, you would at once examine the state of the heart, whether there were any symptoms referable to that organ or not. Let us suppose that you find, as in the majority of cases you will do, a rubbing sound, showing the existence of pericarditis, or a bellows sound, indicative of valvular disease or of extreme anæmia. What are you to do in such a case as this? The patient is quite delirious, tosses about the bed-clothes, talks wildly, tries to get out of bed, and will not be restrained.

Although there is inflammation of the heart here, and none of the brain, yet I can assure you that the symptom which demands most urgently your first attention is the delirium; and that whatever may be your theory as to the relation of the delirium to the affection of the heart, the interests of your patient demand the instant adoption of means to quiet the state of excitement into which he has passed. Important and serious as is the affection of the heart, as compared with the delirium, it is just now of no moment; for if the delirium be allowed to continue, there is great danger of the patient dying suddenly from exhaustion.

Fortunately, however, we have in opium a remedy equally applicable to the cardiac, as well as to the cerebral affection. In these cases you must give opium to produce sleep, just as you would do in traumatic delirium or in delirium tremens. Opium thus given will quiet not only the brain, but the heart also; and it no doubt exercises a favourable influence on the inflamed serous membrane likewise, checking the spread of inflammation and promoting its resolution.

At the same time you will find it useful to apply a blister to the region of the heart, and to promote a free discharge from the blistered surface. But avoid bleeding, whether general or topical, if you wish to get your patient through without untoward symptoms, and reserve all other depressing treatment until the symptoms of delirium have passed away. During delirium you must also be careful to administer support, because the very existence of delirium makes great calls on the powers of the patient, and he therefore requires to be freely supplied with nutritious matter easy of digestion, and with stimulants. You need not be deterred from giving opium, and administering support and stimulants, by the fear lest such treatment should increase the cardiac inflammation; the experience of many cases convinces me that under this plan the heart becomes less irritable, the pulse slower, and the cardiac inflammation becomes circumscribed, and it tends to terminate by resolution or adhesion rather than by the effusion of water. On the other hand, if you persist in an antiphlogistic plan, you keep up or increase delirium, you exhaust the powers of the patient, and effusion is apt to take place into the pericardium, whereby the heart's action may become seriously impeded.

If this treatment be commenced early, and pursued effectually, it is rare indeed for the patient to go wrong.

[Little need be said on the delirium accompanying acute gout, as the remarks upon the rheumatic form apply also to it; but as it has a peculiar tendency to pass into coma, we must not be so ready to administer opium as in the rheumatic affection, but wait and observe if there is any marked tendency to coma.]

The delirium of erysipelas.—The next subject to which I shall call your attention is the mode of treating the delirium of erysipelas. I know no disease the pathology and treatment of which are more worthy of the careful consideration of the practical physician or surgeon than erysipelas, as it is a malady of very frequent occurrence, and very severe and destructive in its nature. The question of the treatment of the delirium of erysipelas involves the consideration of the treatment of erysipelas itself. I refer here of course particularly to the idiopathic erysipelas, which comes under the observation of the physician; but the remarks I have to make will apply equally to the phlegmonous form which you will meet with in the surgical wards, for the two forms of the disease are of essentially the same nature.

Let me begin by stating what appears to me to be the most reasonable view of the nature of erysipelas, and the most consonant with its clinical history. Erysipelas is produced by the introduction into the system of a morbid poison, generated in the body under certain circumstances, or derived from another individual affected with the disease. A patient who has received an injury may generate the poison of erysipelas in his own person by exposure to certain deleterious influences; or he may, under similar circumstances, generate this poison in his own body, even when he has not received any wound, in which case we call the erysipelas *idiopathic*. Or whether wounded or not, he may imbibe the poison from some one labouring under the disease, even without actual contact.

The poison of erysipelas, like that of measles, of small-pox, of scarlet fever, &c., has its special habitat,—that is, it shows a proneness to affect certain tissues to the exclusion of others; and it attacks specially the skin and the gastro-pulmonary mucous membrane, sometimes affecting the throat first, and travelling outwards to the skin; at other times taking the contrary course. This poison produces other effects than mere cutaneous inflammation; its introduction into the blood causes a fever of a very depressing nature, and this amount of depression bears no constant relation to the extent of the cutaneous inflammation. A large dose of this poison may kill a patient in a few hours, causing at the same time only very trifling local disturbance, such as a trifling redness of the throat. Generally speaking, however, it requires many days for its elimination, during which time the patient exhibits the local and constitutional symptoms with which you are familiar.

Like the other morbid poisons to which I have alluded, the poison of erysipelas exhibits secondary and tertiary effects: secondary effects in the formation of abscesses in various parts of the body, and even in parts which may not have been affected by the cutaneous inflammation; and tertiary effects in chronic inflammation and induration of various glands, and the deposition of scrofulous matter.

What is the most reasonable and the best treatment of erysipelas? have we any means of eradicating the poison by a bold stroke at the onset of the disease? Slight cases,—or, in other words, cases which have imbibed only a small dose of the poison—will quickly recover, with little or no medical treatment; and such cases, when treated early by a bold antiphlogistic measure, will appear to have been cut short. But, in the majority of cases, when a large dose of the poison has been imbibed, I believe that the disease must pass through a certain course, that the poison will produce its specific effects, and that the principal business of the practitioner is to uphold the powers of the patient so as to limit as much as possible the destructive influence of the poison. In such a case you can no more think of extracting the poison by some active stroke of treatment, than you can think of eradicating the poison of small-pox or scarlet fever under similar circumstances. Any one who would, now-a-days, think of such a thing in these maladies, would be set down as having lost his senses. We have no means of extracting the poison; we must, therefore, endeavour to guide our patient through the various stages of the fever it excites, into port, or we must find some antidote for it, which, when introduced into the blood, may counteract its depressing and destructive effects.

There is no treatment which appears to me to be so generally applicable and so safe in cases of erysipelas, as that which we call the stimulating treatment—by support by means of nutritious food in the liquid form, and by bark, or ammonia, or chloric ether; nor is there anything which seems, in some cases most strikingly, to act so much as an antidote to the erysipelatous poison, as alcohol, given either as brandy or wine, or beer, but more especially as the first.

In all cases of erysipelas, then, my practice is first to evacuate the bowels, when necessary, by such means as will not occasion any undue depression, and then to give nutritious food, easily digestible, with stimulants, as brandy or wine. I adjust the quantity of these according to the urgency of the symptoms, keeping, however, to this rule—namely, to give small quantities very frequently, and at short intervals, rather than larger quantities administered occasionally. This is, if I may use the expression, the staple of my treatment: to it I add such medicines as ammonia, bark, &c.; but if the stomach be irritable, or the patient opposes or is disgusted with the administration of so many things, I prefer giving up the drugs and trusting to food and brandy only.

Patients treated in this way from the beginning do not often become violently delirious. When delirium sets in in erysipelas, it may be taken as an indication that our patient wants more support and more stimulus; and the more quickly and freely you give them under such circumstances, the sooner you will subdue the delirium. If freely given just at the beginning of the delirium, you may check it completely. Of this I had a remarkable example last summer, in the case of a clergyman, a robust man, who came to town with his family to see the lions. He caught cold, as he thought, from sitting one hot day, with a north-east wind, near an open window in an omnibus. That evening he had a severe rigor, and vomited. I saw him soon after, and found some redness of the mucous membrane of the nose, spreading outwards on the skin. I immediately suspected erysipelas, and the next day confirmed my suspicions, as the redness had extended over the nose. He was moderately purged, and to his surprise I allowed him two or three glasses of port wine at once. Under this treatment he seemed to go on well for a couple of days. One night I was led almost by accident to visit him at a very late hour; and to my surprise he was looking strange, talking rather wildly, and wanting to get up and go out. I immediately called for the brandy bottle, and gave him a large dose of it, which seemed to quiet him; and I left instructions with the nurse to give him smaller quantities at short intervals through the night. Next morning I found that he had slept through the greater part of the night, and that the delirium had completely ceased.

In cases which show a tendency to coma, it is often advisable to shave the head and apply blisters freely to the scalp or back of the neck. Nor need you be deterred from applying blisters, by the fear lest the blistered surface should become the seat of new erysipelas. As a rule, it is not desirable to give opium in the delirium of erysipelas, unless the delirium is decidedly of the active and wakeful kind; on the whole, however, the other narcotics, as henbane, camphor, hop, are safer than opium.

The stimulating treatment in erysipelas not only saves the patient from delirium, or cuts it short when it appears, but it shortens the period of convalescence, and, what is very important, it seems to diminish the tendency to secondary deposits. We have had many cases in the hospital which exemplify this remark. You may recollect a very severe case of erysipelas last winter in Lonsdale-ward: the patient was quite delirious, and there was afterwards a tendency to coma; nevertheless, this patient quickly recovered, and had no secondary abscesses; she was treated by stimulants from the beginning. In Augusta ward, also, we had a similar case about the same time, which was treated in the same way with a like result. The clergyman's case, to which I just now alluded, afforded another example of the absence of secondary deposits under stimulating treatment, although the erysipelas extended over the whole scalp. His wife, on the other hand, who caught the disease from him, had a very irritable stomach, and could not take stimulants; and she had an abscess in one of the upper eyelids. In the case of a young lady whom I attended in the spring, along with Mr. Bowman, with severe erysipelas, there was no secondary abscesses; she was treated by brandy and nourishment from the beginning; and I could enumerate several other instances in which this desirable result followed the stimulating treatment.

Now and then it will happen that an erysipelatous patient will rapidly become comatose, and die in spite of all our remedies. There was a man in Sutherland ward a short time ago, affected with erysipelas, who appeared going on very well for some time, but suddenly he became comatose, and died, and we could not make out any reason for his death. The only reasonable explanation that I can offer of the sudden change for the worse which sometimes occurs in these cases, is derived from the well-known tendency they have to form pus, which, accumulating at some point, may find its way through the ulcerated coats of some small vein, and thus enter the circulation, producing coma and complete prostration.

[Dr. Todd declines entering into any discussion as to the treatment of delirium in scarlet fever and measles, as it is in all essential points the same as that of erysipelas, and requires the same guidance in its management.]

I now proceed to consider the treatment of the delirium of typhus fever. In all low forms of fever delirium frequently manifests itself; and this is particularly the case in the course of typhus fever, and of typhoid fever also, if you choose to adopt the opinion of those who admit the existence of two distinct forms of disease: the one a fever of low type, characterized by a tendency to ulceration of Peyer's glands and diarrhœa --*the typhoid fever;* the other, *the true typhus,* a contagious disease, with brown tongue, great prostration, and without the tendency to diarrhœa. This is not the time to discuss the question whether two such distinct forms of fever really do exist. I must content myself with using the term typhoid delirium generically; for in both states of fever we meet with delirium of the same character, which requires the same kind of treatment, but we notice it under the two following forms:—

1st. Delirium of the low and muttering kind, in which the patient lies on his back, constantly muttering to himself, apparently unconscious of what is going on around him, but capable of being roused by loud speaking.

2ndly. Delirium of the active kind, in which the patient is very restless, wakeful, talkative, wanting to get up, and requiring careful watching, and perhaps restraint.

When the delirium is of the active and wakeful kind, it generally comes on quite suddenly; but the low form of delirium, which is the most common, come on very gradually, being first noticed perhaps at night as a slight wandering, then the next night becoming more marked in its characters, and afterwards continuing through both day and night.

Now delirium in fever, of whatever kind it be, is a very serious symptom, and demands the closest attention from the practitioner. Its occurrence generally denotes great depression of the vital powers, and it is of itself calculated to increase exhaustion, especially if the delirium be of the active kind.

In considering the treatment of the delirium of typhus, we shall do well to look a little into the nature of typhus fever itself, as in a former lecture I referred to the chief points in the nature of erysipelas while discussing the treatment of the delirium which accompanies that disease. Typhus fever, then, may be said to arise from the introduction into the system of a special morbid poison, capable of being generated in the human body, and of being communicated from one person to another, although possibly this may not be the only way in which the poison may be propagated. The severity of the symptoms produced by the imbibition of the poison varies much in different cases, depending upon the dose of the poison which has been imbibed, and perhaps, also, upon the condition of the patient at the time of the imbibition, and likewise upon the nature of the poison itself; for doubtless this may vary at different times, and so give rise to that variety in the nature of different epidemics to which most practical men bear testimony.

The morbid poison is eliminated from the system through certain channels, as the skin and mucous membranes; but, like the poison of erysipelas, scarlatina, and many others, it exerts an influence on the condition of the system, which is generally of a very depressing kind, and this continues until the poison is eliminated, or its influence exhausted by some change which it may undergo in the blood.

This seems to be the view of the nature of typhus fever which is most consonant with reason and experience. If it be true, we can scarcely expect that any means we can devise will cut short the fever, or rapidly eliminate the poison from the system. We must deal with the disease as we would with erysipelas, scarlet-fever, small-pox, and support our patient while the fever runs its course, guarding him as much as we can against the destructive influence of the poison. And as this poison is apt to cause great depression, the chief business of the practitioner will be to devise the best means of opposing this depressing influence. This may be best done by giving food, of a nutritious kind, in such a way as may be most easily digested, and will occasion the least irritation of the digestive organs. It will generally be found necessary to administer stimulants, and I think that it is unwise to postpone this part of the treatment long; on the contrary, I prefer giving stimulants *early*, as, by so doing, the necessity for giving them largely is generally avoided. Throughout the whole course of the fever, the greatest pains should be taken to husband the strength of the patient by good nursing, and by instant attention to every want. He should never be suffered to do anything for himself, nor allowed to get out of bed; he must be raised or turned in bed when necessary, and he must be fed as you would an infant. Too much importance cannot be attached to these apparently trivial matters in the management of so serious a malady as typhus fever. Many a patient has lost his life for want of these necessary attentions,—either from the culpable neglect of them on the part of attendants and nurses, or from his inability to obtain them.

If the bowels are confined we should give a purgative which will act very mildly, or they should be opened by enema; and, in giving medicine for the bowels, we should never lose sight of the danger there is in all these cases of the supervention of a troublesome and debilitating diarrhœa. It never can be necessary in typhus fever to purge, or to do more than keep the bowels in such an open state as will assist digestion.

In consequence of the liability of Peyer's glands to be affected with ulceration, strong purgatives may do great mischief, by increasing the inflammation, and perhaps exciting ulceration, which may end in perforation of the intestine, and fatal peritonitis. On the other hand, it is very necessary to provide for the due evacuation of the intestinal canal, not only to promote the digestive powers and the due absorption of the food, but also because, if the secretions are allowed to remain in the bowels, they will themselves excite irritation, and interfere with the due elimination of the poison.

All men of experience agree that it is right and necessary to uphold the strength in typhus fever, but differ as to the best mode of effecting this object. Some only give nutritious food, others think it necessary to combine stimulants with it. My own experience has led me to the conclusion that stimulants are necessary in the vast majority of cases. I have never seen any ill effects arise from the early administration of stimulants; and, on the other hand, I have often had occasion to regret that the use of them had been too long delayed. Moreover, this fact weighs much with me in inducing me to give stimulants early,—namely, that if a patient seem over-stimulated, nothing is easier than to pull him down, and that pretty quickly; but if he be insufficiently supported and stimulated, it is often of extreme difficulty to build him up: *hic labor, hoc* *opus est.* And with reference to the administration of stimulants, I have only to repeat the rules which I gave you on former occasions. Do not give large quantities at a time; do not embarrass the patient, but begin with small quantities frequently repeated. Give wine first, and then, if necessary, change to brandy, or some other spirit. Chloric æther is one of the best medicinal stimulants: it may be given in half-drachm doses every three or four hours, or more frequently if necessary, alone, or in combination with five or six grains of carbonate of ammonia.

Such is a brief outline of the general plan of treatment to be pursued in typhus fever. If, now, we find that delirium sets in in the course of the fever, what is to be done? Ought this to lead us to alter in any way our mode of treatment, and to adopt new and different measures? Whatever be the nature of the delirium, we must, I think, deal with it as part and parcel of the fever, and view it as an indication of a more depressed state of the system calling for an increased amount of stimulants, or a change in the nature of them, and for greater diligence in the administration of nutritious food.

Is there any necessity for local treatment directed to the head when delirium comes on in the course of typhus fever? Nothing is more certain than that there is no inflammation of the brain in typhus fever, not even when delirium occurs: there is, therefore, no need for active local measures on this account. Some years ago a theory was put forward by a distinguished physician of this town which ascribed typhus fever to inflammation of the brain; and this gained some support from the fact, that inflammation of the brain frequently takes on the characters of typhus fever, as was the case in a patient whose history I related to you in a former lecture. But the experience of nearly all practical men in all countries tends to show that there is no connection between typhus fever and inflammation of the brain.

You will say, however, that, though there is no inflammation there may be congestion. Undoubtedly this is the case; but the congestion is not peculiar to the brain: it is only part of a general congestion which affects the capillary system of the whole body, and is probably due to the presence of the typhus poison in the blood, which weakens the forces by which the blood is moved in that system of vessels. To relieve this congestion, therefore, ought we to resort to local depletion? Upon this point you must be guided by the circumstances of each particular case, bearing in mind that in so enfeebled a state of the capillary circulation the mere taking away of blood is seldom of much use. What is most needed under these circumstances is something to stimulate the capillary circulation, so as to promote the flow of the blood, which tends to stagnate in the fine blood-vessels. Now for this purpose the application of blisters to the shaven scalp is most useful; and you will often find it a better plan to apply several small blisters to different parts of the scalp in succession, than to apply one large one. Sometimes, however, you will not have time to wait for this process; the patient has a strong tendency to coma, or is actively delirious, and at the same time his strength is rapidly on the wane. When this is the case you must apply one large blister all over the scalp; and you will find Dr. Corrigan's plan a very good one—namely, to cut the blister plaster into strips, and to lay them over the scalp as you would strap a man's leg, so as to bring the blistering material into contact with the scalp at every point.

If you should see any clear reasons for taking away blood, you may best gain your object by having it done by expert cupping from the temples. The rapid application of the well-exhausted cupping-glass may act as a stimulus to the capillary circulation, and promote the flow of blood through it.

When the delirium is of the restless and wakeful form, are we to give opium? As a general rule, we must be extremely cautious in the use of opium in typhus fever. We know that opium tends to produce the same effects as the typhoid poison—namely, to favour capillary congestion; therefore, by giving opium, we should only aggravate one of the great evils of the typhoid state. There is another reason why opium should not be administered in fever—namely, that it tends to clog up the secretions, and so to prevent the elimination of the morbid poison, which is an object we are desirous of promoting. Nevertheless, when the state of congestion is not obvious, and the powers of the patient are not very low, I have seen the very best effects derived from the administration of one or two doses of opium well timed.

You will often find it necessary and most useful to give opium by enema in those cases of diarrhœa which often occur in typhus, or, if you will, in typhoid fever. This is a practice which I always follow, and with the best results, never to allow the diarrhœa to get ahead, but to keep it down by the frequent administration of small enemata of starch, sometimes, if the diarrhœa be urgent, repeated twice or thrice a day, with five, ten, or twenty minims of the tincture of opium. Dr. Corrigan, who has had great experience in the treatment of fever in Dublin, states that the application of not more than two or three leeches to the temple is often of great benefit in promoting sleep; and he was first led to adopt this practice by observing that spontaneous hemorrhage from the nose was followed by sleep in some cases. Dr. Graves lays great stress upon the advantages of combining tartar emetic with opium; he says that the antimony prevents the too narcotic effects of the opium, whilst the junction of opium with antimony promotes the sedative influence of the latter, and guards the system against its depressing power.

Are we to be deterred by the occurrence of delirium from the continued use of stimulants? The mere occurrence of delirium need not deter you from the continued use of stimulants: on the contrary, in some cases it should incite you to give them more freely, in larger and more frequent doses. But in all cases their influence upon the pulse will serve you as a useful guide; and if you find that under stimulants the pulse does not quicken, but improves in quality, and more especially if it diminishes in frequency, you may continue the use of stimulants.

[Dr. Todd now speaks of epileptic delirium. It is met with in persons who may or may not have had fits previously, or it may come on before or after the fits, and may, no doubt, be mistaken for, and confounded with, acute mania and phrenitis. It highly important that it should be truly diagnosed, both by negative and positive evidence. The history and absence of certain obvious phenomena, will denote that it is neither erysipelatous, typhoid, pneumonic, *nor* rheumatic; and the absence of certain other symptoms, as pain in the head, sickness, sluggish pulse, and non-existence of the tendency to coma, will show that there is no inflammation of the brain or its membranes. Besides which, we must ascertain that is not delirium tremens, or connected with hysteria. If it cannot be referred to any of these, it is most probable it will be epileptic, and we must then look for the various positive signs to prove it.]

The paroxysm of delirium may be looked upon as a prolonged epileptic fit. It is a fit in which the disturbance of cerebral nutrition is mainly limited to the convolutions of the brain. In the ordinary convulsive fit, the parts of the brain which are affected are probably the tubercula quadrigemina and the hemispheres: in the delirious fit, *without convulsions*, the latter parts alone are affected. In some of the milder cases of epileptic disease we see this isolation of the mental affection and of the convulsive very conspicuously. Thus we observe in some cases that the paroxysm consists only in a momentary loss of consciousness, from which the patient instantaneously recovers; while in others it consists of sudden convulsive starts affecting the upper or lower extremities, or both, and which, when the latter are affected, are sometimes so severe as to throw the patient down. Yet in many of these the patient retains his consciousness perfectly undisturbed. You have examples of both of these states now in the hospital; one in Sutherland ward, in the man whose skin was darkened by a course of nitrate of silver, which he took before his admission. This man has the smaller fits of loss of consciousness, momentary faintings, of which he will sometimes have several in the course of the day. The other case is a lad of Jewish parents, who has the convulsive startings to a very great extent, sometimes fifteen or twenty times a day, and frequently with great violence; but he assures us that never, in even the most severe of them, by which he is thrown down with violence, does he lose his consciousness. This lad, however, has also the regular and fully developed fits, but not more frequently than once in three or four weeks.

It is not, then, unreasonable to suppose that if you may have a short and very temporary affection of the intellect and consciousness, you may also have a prolonged affection of them, constituting the epileptic delirium, which may be ushered in by a regular fit, or which may be determined by a regular fit.

Now I apprehend that no one, now-a-days, will pretend that we have as yet discovered any mode of cutting short the ordinary epileptic convulsive paroxysm. Many means for this purpose have been proposed; such as putting salt in the mouth, pressure on the carotids, bleeding, splashing with cold water, but none have been followed with any degree of success which justifies us in adopting them. I do not say that you may not, if you fancy, try the more harmless of them, such as the salt, and the cold water; but anything which interferes with the circulation is dangerous, and must not be tried on light grounds.

I do not believe, then, (to answer the question which I just now proposed), that we have any sovereign means of cutting short the paroxysm of epileptic delirium; and therefore. I regret to say, that very much of the advice I have to give you on the management of these cases must consist of cautions to you as to what you ought not to do, rather than of positive instructions as to what you must do.

We have, in fact, in the treatment of a case of this kind, to guide our patient through a prolonged epileptic fit; to support his powers until the excitement of the paroxysm passes off, and to guard him against injury. Time is the great element in his cure, and the clinical history of similar cases gives us the best assurance that the paroxysm will in time, if we do not allow the patient's strength to be exhausted, exhaust itself. Our treatment, then, must be mainly *supporting* and *expectant*, with due attention to the ordinary functions of the digestive organs.

And now I must tell you what you ought not to do.

In the first place, as we have the most satisfactory evidence that there is no inflammation of the brain in these cases, you need not harass your patient by the employment of antiphlogistic remedies. You must not bleed him: there is no necessity nor demand for this practice: it often increases the violence and the duration of the ordinary epileptic paroxysm, and it also tends to increase and prolong the delirium, as it does in other forms of delirium. Neither is it advisable to bleed locally, either by leeches or cupping,—or, if you think it prudent to yield to the solicitations of friends, take care to be very sparing in the quantity of blood you take away. When the pulse is strong and slow, stimulants are not required, your patient will be better without them: but when the pulse is weak, vacillating, irregular, and more especially when it is quick and running, then stimulants carefully given will prove advantageous. You may shave the head; and, if it be hot, apply cold applications, but in such a way as not to depress the heart's action. This will give the patient and his attendants something to do, and enable you to proceed the more comfortably with your expectant plan. You may likewise apply small blisters in succession to the scalp, but do so without irritating the patient much, and desist immediately if it seem to have that effect. The moral treatment is particularly to be noticed here, as in many cases it proves of great benefit; all causes of excitement should be carefully avoided. As to restraint, you must exercise the rules I mentioned in speaking of delirium tremens. All means must be taken to prevent the patient from injuring himself; but the gentler the means employed (provided they be effectual) the better. Upon this point I should be glad, if I had time, to read to you some extracts from Dr. Conolly's valuable lectures on the treatment of acute mania, published in the 'Lancet'; but I advise you to peruse them for yourselves—to read, mark, learn, and inwardly digest the wise and humane cautions there given as to the management of these cases.

Sometimes there is extreme wakefulness, and you cannot get the patient to sleep. What are you to do? Avoid opium, generally speaking: if you give a sedative at all, let it be hyosciamus or hop. Sometimes the cold douche or the shower-bath prove very effectual in inducing sleep. The head may be placed out of bed, and cold water poured upon it from a height, or it may be very freely sluiced with a large sponge. I have seen chloroform of great use where opium had utterly failed: but you must bear in mind the same precautions with regard to its use as to the use of opium; do not give it if you can do without it. Any other drugs which you administer should be of the tonic kind, of which the most useful are the metallic tonics, as zinc and iron, or you may give bark or quinine.

[In speaking of the general treatment of coma, Dr. Todd excludes the apoplectic coma; that is, by effused blood; or, indeed, pressure produced in any other manner upon the brain. And, therefore, in dealing with any particular case we must first satisfy ourselves that it is not coma from pressure, or from inflammation of the brain.]

In investigating cases of coma you must be careful to inquire into the state of the renal and hepatic secretions. When either the liver or the kidneys fail, the patient becomes comatose. The liver may fail either from actual non-elimination,—the elements of the bile remaining in the blood, the liver having lost its power of attracting them out of it,—or there may be some mechanical impediment to the flow of the bile, either in disease of the ducts themselves within the liver, or in some stoppage of the hepatic or common ducts outside the liver. The hepatic derangement shows itself plainly enough in the jaundiced state of the patient. When the kidney is at fault I need not tell you that you will find the evidence of it in a careful chemical microscopical investigation of the urine.

The former history of the patient affords the most valuable and important guidance in the diagnosis of the various forms of comatose affections. You must inquire into the state of the patient previous to the occurrence of the coma, and must consider whether he has been the subject of epilepsy, gout, rheumatism, hysteria, or renal disease, as the coma may arise from any of these conditions. You should also inquire into the previous habits of your patient, as to intemperance, taking opium, &c., as coma may arise from the presence of opium or of alcohol in the system.

You must, then, before you fix upon your line of treatment, be satisfied that the coma is not apoplectic,—that is, from pressure; and also that it is not due to the presence of opium or of alcohol in the system.

Excluding these, the coma may be traumatic, from shock, producing simple concussion of the brain, or it may be epileptic, or renal epileptic, or hysterical, or rheumatic, or gouty. I shall not dwell upon the diagnosis of these forms of coma from each other, but proceed to refer briefly to the treatment of each.

First, then, as to the *traumatic coma*. This is that state which surgeons describe under the name of concussion of the brain. We have unquestionable evidence that it is not a state of inflammation, or of active disease of any kind, but simply one of suspension of the powers of the brain due to the shock occasioned by the injury. We do not know exactly what the precise condition of brain is in this traumatic coma. It is, however, a state analogous to that of sleep, in which the natural actions of the brain are depressed rather than exalted: to use an expression borrowed from the Stock Exchange, they are *below par*.

In the milder cases, as when a patient is simply *stunned*, recovery takes place quickly and perfectly without any medical interference. Why should not this be the case with the more prolonged cases, in which the stunning effects of the injury last considerably longer? Indeed, I do not know why it should not; and I believe that in the great majority of cases this state of coma passes off spontaneously, just as it does in the slighter cases.

Are we, then, in cases of concussion of the brain, to content ourselves with looking on, and to do nothing? I believe opinion is rapidly gaining ground that this expectant method,—this system of non-interference,—is the best. Upon this point, however, I speak with diffidence, and must refer you to the great surgical authorities. I shall only add, that most of those with whom I have conversed on this subject have expressed themselves most favourable to this plan. Among them I may especially refer to my friend and neighbour Mr. Bransby Cooper, whose large experience at Guy's Hospital entitles his opinion to great weight. In conversation upon this subject, he likened the state of coma after concussion to a state of sleep which has a distinctly reparative object and effect.

Treatment of epileptic coma.—The epileptic coma is the most common form of coma we meet with; and here, likewise, the expectant mode of treatment, with moderate purging, answers better than any other. This condition presents many analogies to the traumatic coma. The brain experiences a shock from the epileptic discharge. The shock is generally followed by a longer or shorter sleep, from which the patient awakes up relieved, and often with no other symptom than a feeling of exhaustion. We do not find that anything cuts short the attack. Bleeding depresses the heart's action, and is favourable to the development of the epileptic state, and therefore it cannot tend to cut short the coma.

There are, however, cases of coma in which more active treatment than this is required; as, for instance, in cases of coma arising from rheumatic fever, gout, or scarlet fever, or in coma arising from poisoning by urea, in diseased states of the kidney. In such cases it is very necessary to do something more than watch: your treatment must be of the eliminating kind, such as blistering and purging; at the same time, it is necessary to uphold the powers as far as the digestive organs will admit. When you have reason to believe that the blood is poisoned by urea, as in renal disease, you will frequently find the hot air-bath of service; and you must give drastic purgatives, such as elaterium, or the compound powder of jalap, or the Iberis amara. In fine, as a general rule in the treatment of the comatose state which does not arise from pressure, you must bear in mind what I think I have made out in these lectures,—namely, that delirium is the slighter degree, and coma the more aggravated condition of the same state; hence, in a great measure, the treatment of the two conditions must be similar; for coma and delirium differ from each other in degree, the former being only an advanced stage of the latter affection.—*Medical Gazette, June* 14, *p.* 1031; *June* 21, *p.* 1077; *June* 28, *p.* 1097; *July* 12, *p.* 69; *July* 19, *p.* 109; *and July* 26, 1850, *p.* 133.

16.—ON THE TREATMENT OF CEREBRAL HYSTERIA, AND OF MORAL INSANITY IN WOMEN, BY ELECTRO-GALVANISM.

By Dr. LAYCOCK, Physician to the York Dispensary.

[In describing the characteristics of this peculiar condition, Dr. Laycock remarks:]

There is a disorder of the feelings, sentiments, and affections, which, when intense and permanent, constitutes moral insanity, but, in a minor degree, may be termed cerebral hysteria, and to which women with certain mental and corporeal characteristics are specially liable. It presents itself under the three forms of exaltation, depression, and perversion,—the latter a state often compounded of the two former. It depends on a functional disturbance of the cerebrum, but primarily and principally of that portion which is the organ of the instincts and emotions,—for there are no delusions or hallucinations, and no disorder of the intellect, except in the advanced stage, when the functional disorder has become chronic, and mania, melancholia, or imbecility, has supervened. In its mildest forms, the disease is one of the most common to which the sex is liable, and, as constituting the primary or incubation-stage of moral insanity, it is one of the most distressing. It is also, when fully established, to be classed amongst the most incurable; for, although the intellect may be sufficiently restored to render the restraint of an asylum unnecessary, there often remains a permanent change in the character of the individual of a very distressing kind, because it gives the appearance of moral depravity, and too often really induces it. The gentle, truthful, and self-denying woman has unaccountably become cunning, quarrelsome, selfish; piety has degenerated into hypocrisy, or even vice; and there is no regard for appearances, or for the feelings or interests of others, except in so far as they may minister to the vanity or selfishness of the patient. In short, there is such a complete perversion of the character, that, when compared with the natural disposition and feelings, it can only be considered as a state of disease, and the result of previous changes in the encephalon.

The remedy I have to recommend as prophylactic and curative in cases of this kind is the preserving and systematic application of electro-galvanism to the abdominal and pelvic regions in combination with other means.

[Dr. Laycock divides the cause of the affection into three stages or phases. 1st. What he terms the etiological stage, as that of causation. In this there is general derangement of the health, unusual languor, irregular appetite, epigastric pain, with other dyspeptic symptoms. There is also constipation with irregularities of the uterine functions. As the disease proceeds, all these symptoms are increased, and with them, those also of dysmenorrhœa or amenorrhœa. The countenance, besides assuming a muddy, sallow, and pinched appearance, exhibits a settled expression of anxiety and habitual gloom. The *stage of incubation* may now be said to have commenced, and the cerebral functions begin to be permanently disordered. The patient becomes fickle, impatient, and restless, easily depressed or excited, and puzzling, with the variety and incongruity of her symptoms, her medical adviser and friends. As the 3rd stage gradually steals on, the symptoms become more decided and progressive, and characterised by a peculiar impulsive manner, shewing itself in all her actions,—nay, so disconnected do her ideas seem, that a character of great eccentricity, if not of actual insanity, is liable to be stamped upon her. Although there may have been no illusions or hallucinations hitherto, yet, however, the transition from this state to confirmed insanity, is only a question of time; the peculiar form differing according to circumstances, assuming mania, melancholia, hypochondriasis, or only monomania.]

As the form which the disease assumes in its successive stages is never alike in any two individuals, so its causes vary. Primarily, in all in whom the cerebrum is decidedly involved, there is an existing predisposition to mental disease or cerebral disorder, either hereditary or acquired. Then functional or structural disease of various viscera has a direct incident excitor action on corresponding portions of the cerebrum and cerebro-spinal axis, and so induces local excitement and disorder therein. Thirdly, visceral disease leads to a morbid condition of the blood, so that the nervous system is injuriously acted upon from another quarter, and a general *neuræmic* condition is induced. This condition I have specially considered in my work on the 'Nervous Diseases of Women,' and its relations are very important and extensive. I do not propose to discuss these etiological questions at length, as I have only to trace such a general outline as will enable the reader to recognise the affection, and understand the *modus operandi* of the remedies.

Those who have paid any special attention to mental disorders, must have remarked how frequently diseases of the viscera are incident exciting causes of insanity. It is to be remembered, however, that slight changes in the temper and the feelings, arising from similar causes, are also dependent upon functional disturbance within the cranium, and belong, etiologically and pathologically, although not nosologically, to the class of cerebral diseases involving the mind. Disease of the heart frequently induces a most painful irritability of temper, or a morbid depression; disease of the stomach and liver will cause hypochondriacal feelings, and a depressing sense of anxiety about the future. Irritation of the intestinal canal will induce various forms of cerebral irritation, from epilepsy to mania; while uterine and ovarian irritation, which, in its slightest forms—as when connected with the monthly period in females—almost always exercise more or less influence on the temper and feelings at that time; operate also, indirectly, on the nervous system, by impairing the functions of *the kidneys*, and so rendering the elimination of the urinary salts from the blood imperfect. It is, however, to the direct and permanent morbid action of the ovaria on the cerebrum, that the change in the instincts and feelings is due, which I have described, and which invests the patient's actions with an unfeminine character, perverting all the feelings and sentiments connected with the sexual functions, either directly or indirectly, and so exciting insane cunning, destructiveness, infanticidal impulses, morbid appetites, &c. It is not possible always to say in what this morbid condition of the ovaria consists, nor, in many cases, how it arises, although, in a large proportion, it is evidently connected with displacement, congestion, or ulceration of the uterus or its cervix. Whatever may be its exact nature, when thus complicated with uterine disease, it draws the kidneys, stomach, and intestinal canal into the chain of functional disorder, and these re-acting upon the cerebrum and cerebro-spinal axis, and the latter on the former, we have the whole series of phenomena developed in its various modifications, according to the modifying influences of individual predisposition.

Hence it is of primary importance that the utero-ovarian disease be checked, and that the gastro-intestinal functions be restored to a healthy condition; for, unless these indications be fulfilled, there is little hope that the cerebral or cerebro-spinal disorder thence resulting, will be cured, or even palliated. The various remedies which are in ordinary use for these purposes,—as hip and foot-baths, leeches and counter-irritants to the spine, chalybeates, purgatives, enemata, sea-bathing, &c.,—are uncertain in their results, for, in many cases, we cannot even check the constantly increasing tendency to constipation. Now, it is as an aperient and emmenagogue that electro-galvanism, used in the mode mentioned, is most beneficial in these cases; and, without entering into any theory as to its *modus operandi*, I will state, that it is the most efficient remedy for this purpose that I am acquainted with. In proportion as it fulfils the indications of cure, by restoring the due action of the intestinal canal, and of the kidneys and ovaria, the morbid cerebral phenomena disappear, and the mind returns to its natural state. In one case under my care, in which there was the morbid condition of the will, I have described in a most marked form, and a concomitant painful alteration of the character, the remedy was eminently successful; in three weeks the bowels were moved daily without purgatives or enemata, although these had been necessary for three or four years previously; the patient gained considerably in weight, improved daily in mind, and in four months the menses re-appeared, after having been wholly suspended *for five years*. The latter result was the more remarkable, inasmuch as the patient presented certain external characteristics which usually indicate atrophy of the ovaria.

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Although the use of electro-galvanism as an emmenagogue is no new thing in medicine, it is to Dr. Cumming of Edinburgh that we are indebted for much valuable, and, I think, *new* information as to the class of cases, and the mode in which it may be most advantageously applied. I would particularly refer the reader to Dr. Cumming's paper 'On the Use of Electro-Galvanism in a peculiar Affection of the Mucous Membrane of the Bowels,' published in the 'Medical Gazette' for 7th December last,* and also an Appendix to that paper, in the same Journal for January 18th last, where he will find ample instructions as to the best mode of applying the remedy, the precautions that should be taken, and the adjuvants that should be administered.

I will only add one or two points necessary to be observed in the diagnosis of the early stage of this class of affections. The gastric derangement is the most distressing, and is characterised by a sense of uneasiness and depression at the epigastrium, accompanied by an undefinable anxiety and restlessness, usually the most intense at an early hour in the morning. The bowels are generally much constipated, the tongue scabrous, or presenting a rough, *fissured* appearance, the complexion muddy, the countenance grave, or peevish, or anxious. These symptoms being observed, attention must be directed to the uterine functions; and if leucorrhœa, or symptoms indicative of uterine congestion, displacement, or structural change, or of ovarian irritation, be found to exist, then the gastric or gastro-enteric phenomena must be considered as in etiological relation to the pelvic, and the daily local application of electro-galvanism, in the way pointed out by Dr. Cumming, will be the best remedy. Amongst the adjuvants that should be administered concurrently with its use, tar, in small doses, has been found to be the most effectual,—a remedy that we owe, I should add, to Professor Simpson,—and which is itself an excellent remedy for habitual constipation, without the aid of electro-galvanism.—*Medical Times, July* 20, 1850, *p.* 57.

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* See Retrospect, Vol. XXI., page 165.

17.—ON THE CURE OF EPILEPSY BY THE EXPRESSED JUICE OF THE COTYLEDON UMBILICUS.

By Thomas Salter, Esq., F.L.S., Fellow of the Royal Medical and Chirurgical Society, &c.

[Mr. Salter is seeking to introduce this new remedy to the attention of the profession, and says he has great pleasure to add now the experience of others upon the point, who agree with him as to its great usefulness. Mr. Salter recommends the fresh expressed juice where it can be obtained; but as this is not always easily available, he believes the liquid concentrated extract, prepared by Mr. Hooper, is of equal value. He recommends that great attention be paid to the general health; laying down a system of dietetics and exercise, the disuse of fermented liquors, the use of tepid sponging during the winter months, and cold sponging or shower baths in the summer, at the same time sustaining a proper temperature upon the surface by wearing flannel next the skin; and he then relates two cases as examples of its efficacy.]

Case I.—Joseph Lamport applied to me on the 25th of October, 1849, for the cure of epilepsy. He is a stout, well-made young man, of a florid complexion, 22 years of age, five feet ten inches high, by occupation a gardener. He has been the subject of epilepsy for the last five years: his fits are of frequent occurrence: they sometimes take place as often as twice in the day: a month appears to have been the longest interval that he has experienced between the attacks, but of late they have been more frequent than formerly. He states that they come on without any warning; he is insensible from ten minutes to half an hour: on recovering, he is sometimes sick and vomits, and suffers from weakness and headache, but in a few hours he feels quite well again. His appetite is good, bowels regular, and pulse natural. He was directed to take one drachm of Hooper's fluid extract of cotyledon in water twice a day, and an occasional dose of compound rhubarb pill if the bowels required it. He has continued the medicine pretty regularly up to the present time (May 7th). The benefit he experienced was strikingly marked from his first taking it. He has had no fit for the last three months, and is in every respect at the present time quite well.

Case II.—On the 21st of October, 1849, I was consulted by a gentleman of about 20 years of age, who had been for some time the subject of epilepsy; but, from the fits hitherto occurring in the night, it was not known precisely how long he might have laboured under the disease. However, about six months since, previous to my seeing him, from the attacks occasionally occurring in the day-time, the complaint was detected. His parents had, however, for a long while suspected that something was the matter, from his sometimes not rising at the usual hour in the morning, and afterwards from his appearing dull and stupid during the day. Moreover, he had often been heard to make strange noises in the night; but, as he had been known to suffer from what is called nightmare, the circumstance did not give rise to the suspicion that he was affected with any serious disorder. This being once ascertained, his bed-room was changed, so that he might be within hearing of his mother.

The intervals between the attacks after this period varied from two to three or four weeks. Occasionally he had only one fit: at other times he would have four or five in succession in the course of one night. I now found on inquiry that he generally knew when the attacks were about to come on, from a peculiar feeling in his left arm and hand which took place the day previous. This was an uneasiness, scarcely amounting to pain, yet clearly allied to it: there was a feeling of stiffness, and an awkward inconvenience when it was moved; a crampy feeling, with occasional twitches of the muscles, and contraction of the fingers, together with some numbness; but he was not aware of any *aura* before the attack. The patient was of small stature, of a nervous and irritable temperament, and considered to be weakly rather than otherwise, but had, notwithstanding, enjoyed good health. The appetite was good; the bowels were disposed to constipation. Before he was put upon a regular course of medicine for the relief of the epilepsy, I thought it right, in the first place, to clear the bowels, and to observe if any common source of irritation existed that might be thought directly or indirectly to have occasioned the fits. He was ordered to take a sufficient quantity of compound rhubarb pill to keep up a full action on the bowels, and to leave off suppers, of which he was accustomed to partake largely. But as the fits continued, and it was suggested that he might possibly have worms, he took at intervals of two or three days several doses of turpentine at night, followed by castor oil in the morning, and on one occasion a large lumbricus was passed. But this treatment had no effect in diminishing the paroxysms; on the contrary, they were thought to be decidedly more violent, particularly on one occasion,—the day following the use of the turpentine. On April 21st he was put upon the use of the cotyledon, and ordered to take a fluid ounce of the recent expressed juice of the plant twice daily. In the mouth of June, from the scarcity of the plant, he commenced the concentrated extract, as prepared by Mr. Hooper, which he continued up to September the 9th, about which time the fits left him, and have not since occurred, having previously gradually declined in frequency and severity. He continues now in good health; but, by his own desire, he still takes the cotyledon as a prophylactic.—*Medical Gazette, June* 14, 1850, *p.* 1025.

18.—*Chloroform in Neuralgia.*—Some forms of neuralgia resist almost all the means used to subdue them, and anæsthetic agents may naturally be expected to be beneficial in such cases, both internally and externally. The 'Journal de Pharmacie' gives the following formula for a chloroform ointment: chloroform, sixty drops; hog's lard, one ounce. Mix in a mortar, and use two or three frictions a day upon the painful spot. As this ointment turns yellow when exposed to the light, it should be placed in a coloured, wide-necked, and well-stoppered bottle.—*Lancet, July* 20, 1850, *p.* 85.

19.—*Case of Sciatica Cured by Cauterising the Ear.*—[However singular it may appear, M. Malgaigne did actually apply the red hot iron to the anterior part of the right helix in a case of sciatica, and his patient was cured forthwith. Similar success is stated to have resulted on more recent occasions at the Hospital St. Louis, and]

Dr. Lucciana of Bastia relates in the 'Journal des Connaissances Médico-Chirurgicales,' 1st May, 1850, a radical mode of curing sciatica, popularly practised in Corsica, and consisting in the application of a red hot iron to the ear, and exactly on its helix. The cauterisation cures the sciatica instantaneously, or at least effects immediate improvement. The operation is in Corsica uniformly performed by the farrier, and the inhabitants, when affected with sciatica, lose no time in applying for his assistance. In confirmation, Lucciana adduces some cases of sciatica which he had in vain attempted to cure in the hospital of Bastia by other therapeutical methods, and which yielded to the farrier's cautery, as if to a charm.

In relation to this kind of cure, and to its results, we recommend the perusal of the article 'Reumatismo' in the 'Instituzioni Chirurgiche di Monteggia,' parag. 499, in which mention is made of a priest "who cured sciatica by a small burn behind the anti-helix. The editor of the journal from which this article is taken, adds other authorities in support of the energetic method of cure revived by Lucciana. He cites Mercatus, who recommends, in sciatica, "to open the vein in the middle of the ear with a hot iron," and who probably took the hint from Hippocrates, who advised, "in fluxes descending along the thigh, to open the veins behind the ear." Zacutus Lucitanus, in the second book of his 'Praxis Medica Admiranda,' has an article entitled 'Ustio venarum retro aures ischiaticis utilissima,' and professes himself well satisfied with the results which he had himself obtained by this method of cure in certain obstinate cases. He also tells how a traveller, who had long been resident in Japan, in his presence cured an obstinate sciatica, after it had resisted scarification of the skin behind the ears, by cauterising the same parts several times within the space of two hours with a hot brand of lignum vitæ.—*Monthly Journal of Med. Science, Aug.,* 1850, *p.* 173.

20.—*Citrate of Caffeine in Hemicrania.*—Dr. HANNON advocates very strenuously the use of the above salt in hemicrania. We find in the 'Presse Médicale,' of Brussels, two long articles on the subject, with some cases and a good description of the caffeine and its preparation. It is to be given in doses, varying from five grains to one drachm; and the author has added formulæ for administering the citrate in the shape of pills, syrup, draught, lozenges, &c.—*Lancet, Oct.* 19, 1850, *p.* 451.

21.—*Illustration of the Difficulties which beset the Diagnosis of some Cases of Disease.* By THOS. POYSER, Esq.—*Obscure Neuralgic Pains: Disease of the Brain, and Enlargement of the Liver.*—Mrs. —, aged 40, the mother of a large family, and a lady of great intellectual attainments, who had usually been in the enjoyment of good health, was attacked in the autumn of 1844 with acute pain in the right groin, or rather in the inner and upper margin of the ilium. The pain was limited in extent, and might be covered, as she expressed it, with her finger, but was so acute as to deprive her almost entirely of sleep, and to render any motion of the part nearly insupportable. There was no swelling, redness, nor any appearance of inflammation. This pain continued some months, without varying its seat or character, and but little controlled by treatment.

It was considered to be neuralgic; and every means that could be suggested, both constitutional and local, were tried by an eminent surgeon who then attended the case. During the progress of this affection, Mrs. — herself discovered, while in bed, (to which she had long been confined by the neuralgic pain), a swelling in the right side below the liver. On examination, this viscus was found much enlarged, extending four or five inches below its normal size and situation, of a stony hardness; but unattended with pain or tenderness on pressure. It seemed to account for the pelvic pain, inasmuch as the pressure from this indurated mass on the nerves might occasion it. Mercury with taraxacum, and iodine and mercurial frictions, were therefore had recourse to, without any alleviation of the pain, or diminution of the hepatic disease.

In the spring of 1845, the patient was removed to her mother's house in this neighbourhood, and on April 5th I saw her. In addition to the enlargement of the liver, and the acute pain on the inner margin of the crista ilii, there was now tympanitis, the bowels being enormously distended with flatus. There was no pain in the head, no fever, and the pulse and tongue were natural: but there was a degree of irritability of mind and fretfulness which was remarkable, as Mrs. —'s natural disposition was peculiarly placid and gentle. It is unnecessary to detail the treatment, as but little or no relief was obtained from it.

The tinctura cannabis indicæ soothed and tranquillized the nervous feelings; and the unguentum aconiti as a local application, (suggested by Sir Benjamin Brodie, who was consulted,) in some degree assuaged the pain; but the general character of the symptoms continued unabated, till about the middle of April, when the pain began to diminish, and, in two or three days, went off altogether. After the removal of the pain, Mrs. —'s spirits and appetite improved; she was able to sit up and walk about her room; and although the tympanitic swelling and enlargement of the liver did not diminish, yet she was cheered by the freedom from pain, and the prospect of recovery.

On the 28th of April, while sitting in her chair, Mrs. — had an apoplectic seizure, which deprived her of speech and the power of swallowing, from which she never rallied. She expired May 3rd.

A *post-mortem examination* was made thirty-six hours after death. On removing the calvarium, the external part of the brain presented no diseased appearance; but, on cutting into the left hemisphere, it was found to be quite degenerated in structure. The whole of the anterior lobe was converted into a thick purulent fluid; and, in the middle of this large abscess, there was a clot of blood the size of a walnut. This coagulum appeared to be of recent formation. The left middle lobe was pulpy and diseased, but the other parts of the cerebrum and cerebellum had a firm consistence and healthy structure. The abdomen was very large, from the bowels being enormously distended with flatus, but there was no extravasated fluid or gas in the cavity. The left lobe of the liver presented a very diseased appearance. It was enlarged to more than twice its usual size, had almost a stony hardness, was mottled and tuberculated externally, and on cutting into it, its texture was gristly and exhibited that diseased structure termed mammary sarcoma. The inner edge of the ilium, where the pain had existed so many months, was carefully examined, but there was no morbid appearance discoverable in the part, or in the nerves leading to it. The thorax was not opened.

Remarks.—This case is, I think, interesting and worthy to be recorded. It shows that the symptoms did not point to the true seat of disease, till a short time before the patient's death; and that extensive disorganization may be going on in the brain, without impairing its functions, or manifesting those signs by which it may be detected. The fretful and altered manner of the patient led to the apprehension that softening of the brain might be going on; but the total absence of pain in the head, and of rigors, or any indication of inflammatory action, rendered the diagnosis very difficult. It is highly probable, that the pain in the right side of the pelvis was occasioned by disease of the left side of the brain, and furnishes an additional instance to those recorded by Sir Henry Halford, where neuralgia was the effect of cerebral disorganization. When the latter disease had so far advanced as to disqualify the nerves from suffering so exquisitely, then the pain ceased. This case shows the importance of attending to the brain, in all long-continued and obscure neuralgic pains, although the functions of that organ may not be impaired.—*London Journal of Medicine, August,* 1850, *p.* 731.

22.—ON THE TREATMENT OF TRAUMATIC TETANUS.

By H. R. De Ricci, Licentiate of the College of Surgeons in Ireland.

[Mr. De Ricci thinks, in cases of tetanus, we are justified with a view of removing the exciting cause to amputate the entire foot, or to divide the nerves leading to it. We must also endeavour to check the disordered action of the nervous system, by equalizing the powers of the brain with those of the excito-motory apparatus on the one hand, and rendering the muscles incapable of obeying the erratic dictates of the spinal cord, on the other. For in this disease the energies of the brain are minus, while those of the spinal cord are plus. The first we effect by stimulants, such as wine, brandy, and especially the resin of indian hemp, which possesses the property of exciting the brain independently of other parts of the human frame; and the second by tobacco enemata and fomentations. Although the latter remedy is dangerous in unskilful hands, yet under careful restrictions it is highly valuable, the muscles under its influence gradually relaxing from their rigidity, and becoming unable to execute any automatic movement. By these measures, continues Mr. De Ricci,]

Our patient is now in a state of comparative ease; his brain is exalted in its energies, and thus enabled to counteract the spasmodic motions of the spinal cord; and, when it fails, the paralysed condition of the muscles comes to its aid, by refusing to act in compliance with the impulse. The pain at the ensiform cartilage too is relieved, and the patient's forces are no longer exhausted by overpowering convulsions. When once the patient is brought to this condition, his safety is almost a matter of certainty; for what is the cause of death in tetanus? it is either exhaustion or asphyxia, and not any specific action or morbid poison inherent to the disease itself. Asphyxia is caused by the persistent rigidity of the respiratory muscles; exhaustion, by the repeated attacks of spasm. But, by the treatment I suggest, both these conditions are done away with, the sufferer is relieved from any immediate danger of impending death, and his nervous system has now time to correct the morbid functional condition into which it had lapsed, and by degrees will resume its normal functions.

[In proof of the practical value of his theoretical views, Mr. De Ricci subjoins the particulars of the following cases:]

Patrick Carroll, aged 25, stone-mason by trade, was admitted into Parsonstown Infirmary on the 20th of July, 1841, with a lacerated wound of the great toe, caused by the fall of a large stone on it, which had nearly severed the first phalanx, injuring at the same time, though in a less degree, the second and third toes of the same foot. The accident occurred at four o'clock, p.m., and the man was admitted into hospital almost immediately. He got a cathartic mixture, and a spirituous lotion was applied to the wound. At eight o'clock the same day (precisely four hours after the receipt of the injury,) Dr. Waters was sent for to see the man, who, according to the nurse's report, had had several fits during the last half hour. He at once recognised the so-called fits as tetanus, all signs of which were prominently marked. At this period the muscles of the neck, face, and abdomen, were *permanently rigid;* and the spasms violent, and recurring at intervals of less than two minutes. He complained of soreness and stiffness about the throat, and difficulty of swallowing; he was bathed in perspiration, and greatly alarmed about himself. His pulse was 130, small, and the characteristic risus sardonicus was strongly marked. He was ordered tobacco fomentations over the abdomen, and a tobacco enema (fifteen grains of tobacco to eight ounces of boiling water) every half hour, so as to keep up a state of nausea, this to be thrown up with O'Beirne's tube.

20th July, 10 p.m., *six hours after the accident,* he was greatly prostrated, and complained of deadly sickness; he had vomited large quantities of a dark green fluid; the spasms were as violent as ever, and the muscles rather more rigid. Emprosthotonos strongly marked; pulse 100, very small. Ordered to continue the fomentations and enemata, the frequency of the latter to be regulated by the effect, so that a constant nausea should be kept up. A liniment of croton oil and turpentine was also ordered to be rubbed along the spine, until a free eruption was produced.

21st, 10 o'clock a.m., *eighteen hours after the accident;* during the night he has had twelve enemata, and the fomentations have been continued uninterruptedly; thus the nausea has been maintained. As his bowels had not been freed, he was ordered two drops of croton oil.

21st, 10 o'clock, p.m., *twenty-eight hours after the accident;* spasms less violent, and the interval between them increased to five minutes. The patient is now for the first time inclined to sleep, but is prevented by the recurrence of the spasms; he swallows with difficulty; pulse 100, very small. Nausea continues under the incessant administration of the tobacco enemata and fomentations; he, however, expresses himself as being better. His bowels not having been acted on, he was ordered three drops of croton oil, to be taken at once.

22nd, 10 o'clock a.m., *forty-two hours after the accident.* The patient is considerably better; spasms are much less violent, and the interval between them is increased to ten minutes. The muscles are still very rigid, but he complains less of difficulty in deglutition. Bowels well acted on.

22nd, 9 o'clock, p.m., *fifty-three hours after the accident.* I was disagreeably surprised at finding the patient much worse, the spasms had considerably increased in violence, and the interval was again reduced to five minutes. He has great anxiety, no inclination to sleep, great difficulty in swallowing, intense rigidity of abdominal muscles; pulse 130, very small. He was ordered the croton oil and turpentine liniment again to the spine, and the tobacco fomentations and enemata to be continued incessantly.

23rd. 10 o'clock a.m., *sixty-six hours after the accident.* I found him much better this morning, the rigidity of the muscles considerably diminished, spasms less violent, and the interval increased to ten minutes; deglutition less difficult; pulse 100; bowels well freed.

23rd, 9 o'clock p m., *seventy-seven hours after the accident.* Continues better; spasms less violent; rigidity of muscles is diminishing; easier deglutition; pulse improved; bowels free. Ordered a tobacco enema every third hour only.

24th, 10 o'clock a.m., *ninety hours after the accident.* Much better this morning; he has slept well during the night; swallows well; rigidity of abdominal muscles almost disappeared, and he declares himself quite well. All medicines were now suspended, and he rapidly recovered During the course of his illness, he got plenty of wine, brandy, and beef-tea. The wound constantly presented a healthy appearance.

In the foregoing case, which was one of the most acute on record, death was averted by means of tobacco and stimulants; I do not say cured, as I consider that was effected by nature. He had, however, a very narrow escape on the recurrence, or rather the exacerbation of the symptoms fifty hours after the accident; and I am firmly convinced that he would not have run that risk, had his lacerated toes been remove on his admission into the hospital. He recovered, however, perfectly, and lived till last summer, when he fell a victim to the cholera.

These are, therefore, the conclusions I come to with regard to the treatment of this disease:—to endeavour, if possible, to combat it at its outset, and commence by removing the existing cause, either by excision, amputation, or division of the nerve leading to the part; to give stimulants, such as brandy, wine, and the tincture of the resin of Indian hemp, in doses of from ten to twenty drops every half hour, so as to produce a slight degree of cerebral excitement; to support the patient at the same time by giving beef-tea and eggs; to pay attention to the state of the bowels, and act upon them with croton oil or any other powerful purgative, as in this disease, as is well known, they are extremely torpid and sluggish; and to bring him under the influence of tobacco as rapidly as possible. These are the principal means upon which we have to rely; but, at the same time, I must not omit to warn every practitioner against the use of opium, belladonna, and every other narcotic, which it has been the fashion to administer indiscriminately in every case of tetanus, both in hospital and in private practice, not so much with the view of curing the patient as of stupefying his intellect, so as to render his exit from this world less painful.

And now, before I conclude, let me add a few words concerning those individuals who, in consequence of some idiosyncrasy, cannot be subjected to the continued influence of tobacco without incurring extreme danger. Those possessing such an idiosyncrasy are easily distinguished, as a few minutes after the administration of the first dose symptoms of the most alarming nature will arise: the countenance will assume a deadly hue and a ghastly appearance, and the pulse at the wrist will become quivering and intermittent. I need scarcely say that in such a case the administration of tobacco should be at once stopped, ammonia applied to the patient's nostrils, cold water sprinkled on his face, and every means used for the excitement of cardiac action.

In these cases, after having discarded the use of tobacco, the main reliance is to be placed in vapour baths, which should be applied without removing the patient from his bed, and continued for a considerable time. The patient will invariably express himself as much relieved by them, and they generally diminish the rigidity of the muscles in a very considerable degree, at the same time that they sooth the patient and allay the spasms.

By adopting such a method of treatment as I have endeavoured to describe, we shall not, as I said before, cure *every* case of acute traumatic tetanus, but we shall certainly give the unfortunate sufferers a much better chance of recovery than by abandoning them to the old routine treatment of calomel, opium, and belladonna.—*Dub. Quarterly Journal, Aug.* 1850, *p.* 64.

23.—*Traumatic Tetanus cured by Frictions with Tincture of Belladonna.*—[The following case is from the 'Gazette Médicale':]—M. Bresse, surgeon at the Military Hospital at Rennes, proposed, in 1848, the treatment of traumatic tetanus by the application of tincture of belladonna, and reported a case, in which it had been successfully employed, in the 'Gazette Médicale de Paris' of Sept. 30, 1848. M. Bresse has now placed on record another case which has come under his notice. The patient, one of the *Garde Mobile*, received a wound on the 20th of of March; tetanic symptoms appeared on the 5th of April. Frictions of belladonna were commenced on the 6th, and by the 12th the patient was out of danger. Imprudently exposing himself to cold, the tetanic symptoms returned in the muscles of the back, but were quickly removed by again having recourse to the frictions.

The tincture employed was composed of five parts of extract to eleven of alcohol, and was applied all over the body, and more particularly over the rigid parts.

M. Bresse adds, that another practitioner has arrested trismus, which he feared would proceed to general tetanus, by the same means.—*Med. Gazette, August* 9, 1850, *p.* 263.

24.—ON CATALEPSY, OR TRANCE.

By Dr. Ebenezer Milner, Edinburgh.

[With regard to the pathology of catalepsy, Dr. Milner remarks—]

Catalepsy is undoubtedly more a lesion of function than of structure. The close approximation of its symptoms to severe hysteria shows that those two morbid affections are closely related; and the circumstance that the paroxysms of either are produced by the same causes, would tend to support the hypothesis that they are merely forms of one and the same disease. In hysteria the muscles are convulsed clonically,—that is, with motion; in true catalepsy they are convulsed tonically,—that is, without motion; and in this rests the principal difference between the two.

The nervous system, or rather centres, have their functions in abeyance during the paroxysm,—there is, in fact, a triple lesion of sensibility, of intelligence, and of muscular motion.

Although it is impossible to define or determine the nature of the cerebral disorder in this disease, in true cases of simple catalepsy, in consequence of but few cases terminating fatally, yet it has been noticed, that the state of congestion of the brain is distinctly traceable where individuals have died of catalepsy complicated with mania. In two cases of this kind, given in the 'Dictionnaire de Medicine,' the brain was found in one much injected with blood; the grey matter on the surface of the convolutions was firm and had a roseate hue; the medullary substance presented a large number of mouths of vessels gorged with blood; and the *corpus callosum* was softer than usual. In the other case, the grey matter was observed to be of a dark colour, approaching that of violet, and the medullary substance presented the *puncta vasculosa* large, numerous, and filled with blood. It must be remembered, however, that the same pathological phenomena are observed after death from true uncomplicated mania.

Diagnosis.—This disease has been confounded with hysteria, ecstasy, asphyxia, apoplexy and syncope, tetanus, and the state of death. The older physicians also mistook death from cold for catalepsy, and described cases of soldiers in a state of catalepsy riding into the camp on horseback, firmly seated like statues on their saddles. Here, death from intense cold formed a source of error.

Although our knowledge of the distinctive marks of the diseases liable to be mistaken for trance is now so great that no physician ought to fall into error, yet it is necessary to notice briefly the signs by which a correct diagnosis may be made.

1. From true ecstasy, catalepsy is distinguished by the patient labouring under the former disease being occupied in profound and sustained meditations. The faculty of thought, instead of being annihilated or suspended, is found exclusively directed towards the contemplation of a single object, entirely absorbed with one idea, with some imaginary pleasant object, the powers of imagination being augmented by an enthusiastic exultation. Again, there are no convulsive movements, no rigidity, partial or complete, of the muscles; and, above all, the limbs, if placed in any position by the physician, do not retain that position as they will in simple, and oftentimes in complicated, catalepsy.

2. Catalepsy is distinguished from hysteria by signs which have been noticed when speaking of the pathological nature of the disease. In hysteria the muscular system is oftentimes strongly convulsed, and motions are performed, but the most distinctive sign is the non-retention of any posture in which the patient may be placed.

3. Asphyxia is accompanied with suspension of the functions of respiration and circulation; the countenance is also ordinarily livid and swollen, the mucous membrane of the lips having a very dark hue.

4. Syncope is distinguished from catalepsy by the state of the muscular system, the limbs being extremely flexible,—and this appertains also in a great degree to asphyxia,—and by the general pallor of the countenance and surface of the body, consequent on the suspension of the heart's action.

5. Apoplexy is known from it by the stertorous or sonorous breathing, by the more or less profound lethargy, by the lax state of all the muscles of the body, and also by the co-existence of paralysis.

6. Although tetanus has been mistaken for catalepsy, in consequence of the tension and rigidity of the muscles, yet, the fact that neither sensibility nor the intellectual functions are interfered with, should have prevented such a mistake from being committed.

7. Patients labouring under an intense and prolonged paroxysm of catalepsy have been supposed to be dead, and have been interred alive.

There are numerous cases of this kind on record, and many more where the individuals, after being laid in their coffins, have fortunately recovered from the attack before the period of interment. In such cases, the respiration is insensible, and the heart's action is almost in abeyance; the surface of the body is nearly cold, and presents the pallor of death; and the articulations are stiff. Although it is no doubt a difficult task to distinguish this state of trance from the state of death, yet a careful examination of the body, and, time, would lead to a correct diagnosis. The limbs after death are first lax, then stiff, and ultimately lax again. The stiffness of the limbs, known as the cadavern rigidity, or *rigor mortis*, lasts for a longer or shorter time, according to circumstances; the sooner it supervenes, the shorter is its duration, and conversely. Now the stiffness of the limbs accompanying this intense form of trance, supervenes at once, and lasts as long as the paroxysm continues. This is consequently a valuable diagnostic sign.

Again, as the heart's action most certainly continues in a slight degree during the attack, the stethoscope may be used with advantage to detect the impulse and murmurs. The state of the eyes and the expression of the countenance, as well as the circumstance that the temperature of the body may be sustained in a slight degree, also furnish means by which a diagnosis may be made.

If pressure be made on the eye-ball a few hours after death, the cornea becomes opaque; and this occurs invariably. But if the least spark of life remain, that effect would not be produced. This may, therefore, be said to be a sign at once distinguishing the state of trance from that of death, -one of ready application, and thus rendering wholly unnecessary the carrying out of the suggestion that inhumation should never be proceeded with until the body has shown unmistakeable signs of decomposition.

Catalepsy is a disease which is often feigned, sometimes for the purpose of exciting the sympathies of the charitable, occasionally by soldiers to procure their discharge from the army, often by females in good circumstances merely from a desire of creating an interest in their behalf, and by itinerating mesmeric impostors. To detect the imposition is oftentimes a matter of great difficulty, all the symptoms and signs of trance being exhibited with great truth. Such persons require to be carefully and vigilantly watched, and their character enquired into, when some inconsistencies will be noticed, and their imposition detected. Perhaps the inhalation of chloroform, where malingering is suspected and is necessary to be known, would be of great service. However slowly the individual breathed, the anæsthetic effect of this fluid would be produced; and then if he did not speak or move while the effect lasted, it is more than probable that he would do so on recovery. This is merely thrown out as a suggestion, which I believe may be used successfully in most cases of malingering.

[On the whole it would appear that catalepsy is not be esteemed a dangerous disease, although many cases terminating fatally are recorded; but these were for the most part complicated with other diseases. With regard to the treatment, Dr. Milner says the cold-bath is injurious, but hot-baths, and pediluvia have been found useful. The most effective means, however, seems to be hot pediluvia and the cold douche applied to the head at the same time. The patient cannot swallow medicines; but if there should be any sensibility remaining, ammonia and other stimulants might be applied held near the nostrils. Friction of the surface of the body with rubefacients might be useful. Stimulating enemata might also be of service by relieving the bowels, and acting as a stimulant to the circulation in the parts. It is during the interval between the attacks that therapeutic remedies are likely to be of any avail. The treatment may be summed up in a few words,—the cause must be searched for and removed, if possible. If a hemorrhoidal hemorrhage has been suppressed, leeches must be applied round the anus. If there are worms in the intestines, anthelmintics must be given; or if it be caused by the retropulsion of an exanthematous eruption everything must be done which can remove the irritation caused thereby.]—*Edin. Med. and Surg. Journal, Oct.* 1, 1850, *p,* 328.

DISEASES OF THE ORGANS OF CIRCULATION.

25.—FATTY DISEASES OF THE HEART.

By Dr. Richard Quain, Physician to the Hospital for Consumption and Diseases of the Chest.

There are two forms under which fat presents itself as a disease of the heart. In one of these forms the fat, composed of large cells containing oil, identical with the fat found in other parts of the body, grows upon and extends over the surface of the heart; it then encroaches on, and insinuates itself between, the muscular fibres, in some cases to such an extent as to completely conceal them when the examination is made with the unassisted eye. In the other form of disease, a fatty matter, composed of granules and small oil-globules, occupies and fills the sheath of what was previously muscular fibre. In the one form of disease fatty tissue *grows* on and outside the fibre, in the other the muscular fibre itself *degenerates* into molecular fatty matter. A distinction thus broadly marked in the anatomical characters of these two forms of disease, and in their mode of origin, requires to be clearly expressed in the designations given them. It will, therefore, be well to apply the term *fatty growth* to all cases in which fat tissue constitutes the morbid condition, even though it may encroach so far on the muscular fibres as to cause their apparent transformation into fat; whilst the term *fatty degeneration* will be limited, as in strictness it should be, to those cases in which the muscular fibre degenerates into molecular fatty matter. The necessity for this distinction will be evident as we proceed.

[Speaking of the fatty growth on the heart, Dr. Quain remarks, that it is ascertained with some degree of certainty, that from any circumstances which will promote the formation of fat in general, the heart will partake largely of the accumulation. We must be content in knowing that the material of the fatty tissue must be derived directly or indirectly from the blood, and that the quantity of the one must bear a relation to the supply of material from the other. Dr. Quain continues:]

We know that the quantity of fatty matter in the blood is in direct relation with that supplied by the food on the one hand, and on the other with the greater or less freedom which exists for the free elimination of its elements in the respiratory process. We, therefore, feel, that if the food is deficient in the materials which readily constitute fat, and the respiratory process is accelerated and increased by abundant exercise, little fat can be formed. Equally true is the converse, that those who fare luxuriously and lead sedentary lives will grow fat. In the one case, neither the body nor the heart will suffer; in the other, in the general accumulation, the heart will certainly participate. Beyond these general principles I fear we cannot go, and even to these there are exceptions.

Fatty degeneration of the Fibre itself.—In the following observations I seek to establish the fact, that the molecular fatty matter in the fibre is the result of a chemical or physical change in the composition of the muscular tissue itself, independent of those processes which we call vital.

Anatomists are well acquainted with the substance named adipocere, and with the facility with which it frequently forms in dead animal tissues excluded from the air and exposed to moisture. Much difference of opinion has existed amongst chemists as to the origin of this substance. All admit the great quantity of fatty matter in its composition. One class, however, which includes Gay-Lussac, and Berzelius, believes that "the compound results entirely from the fat originally present in the substance, and that the fibrin is completely destroyed by putrefaction." The other class, which includes Dr. Thomas Thomson and Mr. Brande, believes "that the fatty matter is an actual product of the decay, and not merely an educt or residue." The correctness of the latter opinion' is shown by the following observations: A piece of this substance, obtained from the muscular part of the thigh of a horse, (portions of the specimen are in the British Museum and in that of the Royal College of Surgeons,) was submitted to examination. In its general appearance it resembles spermaceti. It is rather darker coloured and more fragile. Its surface is marked by irregular outlines of a red colour, evidently the boundaries of the fasciculi of the muscular fibres; it emits a strong ammoniacal odour, floats on water, and is nearly all dissolved in ether. The ether, when allowed to evaporate, leaves a large quantity of granular and fatty matter.

The trifling undissolved residue presents, when floating in water, a delicate flocculent appearance. Examined with the microscope, the flocculent matter is found composed of delicate filaments forming a cellular web. In some parts the filaments presented something of a longitudinal arrangement, in others the web appeared perforated by regular foramina.

A microscopic section of the adipocere itself, unacted on by any agent, (the section being made with a knife slightly warmed, required by the fragility of the substance,) showed the following appearances:—1st. Bands running longitudinally, as in the arrangement of the muscular fibres. These bands are dark coloured and opaque, except at the broken edges or extremities; at these points they are seen to be composed of small crystalline scales. The wavy outlines of the blood-vessels or nerves are sometimes seen between the fibres or fasciculi. In the intervals may be observed flat discs composed of radiating acicular crystals. These appearances vanished on putting ether on the specimen, and the merest particle of the filamentous substance remained.

From these observations I conclude, that whatever be the nature of the process, the places of the muscular fibres, the blood-vessels and nerves are occupied by fatty matter, which could not have existed in them during life. This opinion is confirmed by experiments on what may be called the artificial formation of adipocere. Mr. Gibbes, whose communications to the Royal Society have been just mentioned, states that Lord Bacon had indicated the mode of converting the waste flesh of animals into fat, which might be used for various domestic purposes. Mr. Gibbes, acting on this idea, succeeded by different processes, including exposure to running water, to nitrous acid, &c., in converting large quantities of animal flesh, even the carcase of a whole cow, into fatty matter. The difficulty which the experimenter met with in freeing this substance from colour and smell, or his death, appears to have prevented the communication of his further proceedings, which he had promised to the Society, and neither his nor Lord Bacon's discoveries have been made useful in supplying fat from waste flesh. To the pathologist they afford information of peculiar interest in connection with the following experiments:—A little more than two years ago I obtained, for the purpose of examining the healthy structure of the tissue, the healthy heart of a healthy child who had died a few hours previously from the shock of a severe burn. Having satisfied myself on the required point, I placed the specimen in weak spirit and water (one part to eight or nine,) for the purpose of future examination. On looking at the specimen after a few weeks, I found it greatly changed; it had a confused granular aspect, to such an extent, indeed, as to lead me to feel, that there must be some error in previous observations on the specimens of true degeneration, as portions of a healthy heart now exhibited characters so similar. I found, however, that this change existed in all parts of a heart, which I had no doubt had been healthy, in some parts at least, when previously examined. I mentioned the observation to Dr. Williams, and was gratified at hearing from him, that the fact of this change was an illustration of an experiment which he had suggested to his clinical assistant, Mr. Edward Palmer, some time previously, viz., to try whether fatty matter was not formed after death by a molecular change in animal tissues kept excluded from the air but exposed to moisture. He suggested further observations of this specimen, and some further experiments. These have been made, and the result has been, that the heart first described presents the most marked and universal character of true fatty degeneration. It is of a pale cream colour, it is soft, portions of it float in water, and when examined by the microscope exhibit the most advanced state of degeneration, differing from that which occurs during life in this respect alone,—that it is universal. Every fibre is fatty, whilst in that degeneration which occurs during life, many fibres retain their healthy organization.

A specimen of a sound sheep's heart, placed in very diluted nitric acid, has been traced through a series of similar changes; but as a shorter interval has elapsed, it has reached a less advanced stage. The specimens have been examined by Dr. Williams, Mr. Quekett, and Mr. Toynbee, and others. The blood-vessels and nerves participate in this change. It does not appear necessary to illustrate further the conversion of these tissues after death, external to the body, into fatty matter,—we shall, therefore, turn to inquire whether like changes do not occur within the body and before death. Dr. Babington has apparently observed the formation of fatty matter in the blood at the expense of the albumen. Dr. Williams, in the first edition of his 'Principles of Medicine,' recognises the presence "of fat in softened tubercle, pus, atheroma, and gangrene of the lungs, as being derived from the debris of animal matter, as in the conversion of flesh into adipocerous matter." In a subsequent edition of the same valuable work, further illustrations of this process, and of the circumstances under which it occurs, are clearly given. M. Rokitansky mentions several instances in which fat may be found (as in masses of fibrin, in non-vascular tumours, in the walls of arteries, &c.,) in situations in which its presence could not be deemed the result of a deposition from the blood. Mr. Paget mentions a striking example of this change observed by him in some masses of what had been medullary cancer of the liver, "certain parts of which, when examined, were found to consist almost entirely of oil globules." I have seen the same appearance in the central softened portion of a large mass of tubercle from the kidney, the fatty globules being there infinitely more numerous than in those parts where the softening had not commenced. Dr. Copland has recently communicated to me the particulars of a case of chronic inflammation of the membranes, covering the spinal cord, which caused paraplegia. The patient finally died from the paralysis extending to the muscles of respiration. After death, the recent effusion of plastic matter which existed on the upper part of the cord was found to pass inferiorly where the inflammation had first commenced and ceased, into a soft fatty matter. The same learned authority mentioned to me at the same time another case, in which the central portions of some large bands of adhesions, found in a case of chronic peritonitis, consisted of fat.

I find a case recorded without particular reference to this point by Dr. Archibald Hall, in which the fibrin of a hemorrhagic effusion into the theca vertebralis consequent on a fall was found, after an illness of some months' duration, changed into fat. The fatty matter here formed independently of a direct connection with the vascular system, is, at least in such of the cases as have been examined with the microscope, and they have all been but the two last-mentioned, found to be in all its characters analogous to the granular fatty matter observed not only in degenerated muscular fibre, but in various other organs which are the seat of fatty degeneration. Here, then, a most important question arises,—Is the fatty matter which is found in the degenerated muscle of the heart derived from a molecular change in the texture, or is the fat deposited from the blood? In answer to this question we refer to the preceding facts. We have seen the substances analogous to muscle in composition converted both in and out of the body into this same fatty matter. We have traced also the muscular fibre itself undergoing (external to the body, and thus beyond the reach of deposition or the influence of the nutritive process,) a change perfectly identical with that which it undergoes in the living body. There is, therefore, a presumption in favour of the view which holds, that the processes in both cases are identical; that, in fact, when these protein compounds, albumen and fibrin, are effused in a form which is not readily susceptible of organization, when even they enter into the composition of tissues and textures, the organization of which is imperfect from inherent depravity, from natural decay, or from a deficient supply of those elements and influences, in the absence of which nutrition fails, that in these cases I say those substances degenerate and pass into fatty matter. In other words, that when the vital power which belongs to these higher products of animal organization is weakened or destroyed, they yield to the physical and chemical influences which surround them, and by an inherent principle descend into a class which is shared by them in common with plants and minerals. It would be highly interesting to illustrate this argument by facts drawn from the multitude of instances in which different observers have recently found this fatty degeneration. Some of these facts no doubt readily suggest themselves, though, perhaps, less in the case of the heart than in other organs. It is this organ, however, which claims our attention here. An interesting observation made by Mr. Paget supplies a most important step in this inquiry. It is now pretty well established, that a cell germ, or nucleated cell, is an essential element in the nutritive process of all tissues. The first change that can be traced in the process of degeneration of the heart's fibres, and it requires care to observe it, is the destruction of these cell germs in the tissue. Mr. Paget writes: "When the change is least marked, and but little fatty matter has collected within the sarcolemma, the outlines of the nuclei look dim, and they lose their colour. In the further advanced stage, the nucleus of the fibre cannot be seen at all; its former place is indicated, if at all, only by some out of the narrow column of yellow granules, and in a yet later stage, or when the sarcolemma appears nearly full of fatty particles, all trace of both the nucleus and these granules is lost."

Thus we have evidence, that the nutrition of the heart is impaired, and that those powers are weakened, by which it is enabled to resist the influences which tend to the disintegration of its tissue. A review of the circumstances under which this degeneration occurs, is further confirmatory of the view adopted here. These circumstances exhibit impairment of general and local nutrition.

[The most prominent effects of fatty degeneration on the functions of the heart are those which exhibit the deficient powers of the organ. Dr. Quain first considers]

Coma.—Several writers have described coma, preceded or not by giddiness in connection with enfeebled powers of the circulation. Mr. Adams, whose cases have been previously mentioned, observed as many as twenty attacks of coma in one of them. Mr. R. W. Smith and Dr. Stokes have made similar observations. The explanation of these attacks is this, that the power of the heart is reduced, and thus it is rendered incapable of readily sending on the blood which it receives,—hence arises obstruction to the circulation and unequal pressure on the brain. The explanation is no doubt correct.

Syncope.—Cardiac syncope is a term properly and more frequently used by the older writers than by ourselves, notwithstanding the arguments of Bichat on the subject. Dr. Burrows, in his interesting work on the Cerebral Circulation, has clearly established the fact, that syncope is due to a deficiency of that pressure within the skull which is essential to the performance of the functions of the brain. On this principle we can explain the frequency with which this symptom is found to occur in fatty disease of the heart.

The case of an old man who fell under the notice of Dr. Williams and myself is very remarkable. I was one day suddenly called to him, and found him, as I myself and others believed, dead. He had been in his usual state, and taking his dinner a few moments previously. Though neither pulse nor respiration could be perceived, nor in a hurried examination could the sounds of the heart be heard, I felt from something about his appearance that he was not dead. An electro-magnetic apparatus was set to work from the spine to the region of the heart; other stimuli were made use of, and after several minutes he slowly recovered. These fits, in a slighter form, recurred almost daily for two years, when he died in one exactly like the first. The heart exhibited fatty degeneration. In some cases this feeling amounts to nothing more than a sense of faintness, a feeling that the person must fall if he does not lay hold of something; and in some instances, as in a case at present under my care, this slight faintness is accompanied by an impression that he is about to die. Such persons do die. This mode of death is in many cases instantaneous, in other cases death, though sudden, is not so rapid in its occurrence, the fatal faintness is progressive, and death may not occur for several minutes after its onset. Facts within my knowledge lead me to think, that many distinguished men have thus died, and that in this condition has lain the hidden and frequent cause of many sudden deaths. The paleness of the features, the unaltered state of the pupils, the absence of stertor, and the feeble action of the heart, enable us to distinguish these cases from apoplectic seizures when we see them before death. The age also at which apoplexy occurs most frequently, is not that at which we most frequently find fatty degeneration of the heart. For example, of forty-nine cases of apoplexy, forty-two occurred in persons under sixty, and seven only in those over sixty; whereas, in seventy-nine cases of fatty degeneration of the heart, forty-one were above sixty years of age.

Shortness of breathing is recorded as having been present in about one-half the cases. In some cases it appears as a sense of choking or suffocation,—the person feels as if breathing through a sponge. In some instances the difficulty of breathing is so slight, that it is scarcely regarded; in others so severe, that the slightest effort, particularly in mounting ascents, is most distressing. A peculiarity was observed in two subjects, one of which still lives, viz., that reading aloud caused no inconvenience, whilst it was distressing to ascend a gentle height.

[Another of the recorded phenomena connected with fatty degeneration of the heart is *pain.* In some cases it is confined to the region of the heart; in others, it extends over the chest and down the arm, as in angina pectoris; but—]

There is some evidence to show that rupture of a few fibres may occasionally occur without being fatal, and cause pain; but in the majority of cases, the pain appears to be due to over-distension of the cavities with blood on the one hand, or to the spasmodic contraction just alluded to on the other, it being remembered, that though the heart in health does not possess common sensibility, it may in diseased conditions acquire this property. These latter phenomena, that is to say, syncope, breathlessness, and pain may occur separately, or two or all of them may be present at the same time; their combination gives us the characters of the disease known as *syncope anginosa*, or *angina pectoris*. The symptoms or phenomena of this disease were very well recognised, even before Heberden or Rougnon, who wrote at the same time, described them as constituting a special disease. Still, from that period to the present, the immediate condition on which they depend has been a source of great doubt.

This is not the place to discuss all the causes which may produce these effects. For the present, it is sufficient to show, as has been already done, that this degenerated condition of the heart's fibres is a very sufficient source in itself of the several phenomena, (viz. breathlessness, faintness, and pain,) which are recognised under the name of *angina pectoris,* or *syncope anginosa.*

[With regard to the treatment of this fatty degeneration of the heart, Dr. Quain observes:]

We have no evidence to show that we can restore muscular fibres which have been destroyed; all, therefore, that we can hope for in these cases, and that not always, is to arrest, or suspend for a time, the progress of the disease by improving the quality of the blood, and thus supporting the vigour of those portions of the heart still uninjured. The nervous powers being at the same time strengthened, and the blood itself rendered a better stimulant. The effect of treatment calculated to promote these objects is often very striking. I have found attention to the digestive organs, in the first instance, useful; bitter tonics, with alkalies, and subsequently iron in different forms, seemed to act most advantageously, I need not enumerate the habits of life, change of air and scene, dietetic regulations, &c., by which the general health, and with it the part most in need of it, may be improved. In reference to some of the symptoms, I might mention, that I have seen in two or three cases great relief from pain experienced by the repeated application of three or four or more leeches over the region of the heart, followed by a blister. In one case, which I saw with Mr. James W. Illott, of Bromley, some short time ago, attacks of distressing angina ceased for several months, (when the patient died of another disease), after the application of leeches three or four times, and the internal use of iron. Exercise has recently been recommended in the treatment of these affections. The patients cannot take it, if it were desirable that they should do so. If exercise can be taken in the early stages of the disease, it should always be short of producing fatigue.

It may well be borne in mind, that narcotics are given with risk in these cases. One death occurred after an accustomed dose of morphia; another during the inhalation of chloroform. On the other hand, it is needless to indicate the usefulness of antispasmodics in these cases during paroxysmal attacks.—*Medico-Chirurgical Transactions, Vol.* XXIII., *p.* 121.

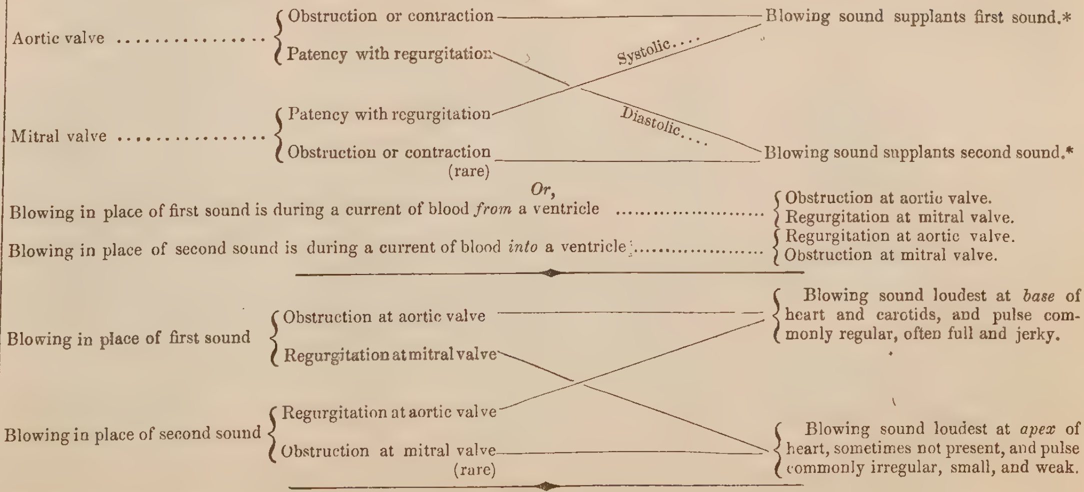
26.—PRACTICAL REMARKS ON DISEASE OF THE HEART, AND THE DROPSY FOLLOWING IT.

By Dr. ALEXANDER KILGOUR, one of the Physicians to the Aberdeen Infirmary.

[Dr. Kilgour states that his conviction, many years ago, was, that those who explained every thing on mechanical principles were apt to refine a little too much, and lay too great stress on a diagnosis, from the abnormal sounds of the heart alone. Nearly seven years' experience, he says, has confirmed him in this opinion, and he only allows that we may often make a pretty good *guess* as to the seat of the disease, but that is nearly all. As an easy mode of impressing in the mind the usual views or guesses, as to the seat of the morbid sounds of the heart, and probable condition of the parts causing them, Dr. Kilgour has drawn up the following diagram (see next page):]

It will be observed that the second form of the diagram is merely another way, so to speak, of stating the same thing as in the first one. But the third is essentially different in its object, having reference to another sign than that depending upon the sounds. Perhaps some will object to the indications derived from the pulse, as not being deserving of so much importance as I am inclined here to attribute to them. I give them, however, only as farther aids to our guesses as to the true seat of the disease; and I must say, for my own part, that, having satisfied myself by my ear, and by such signs and symptoms from the condition of other organs as I can get, that heart disease does exist, I rely more on the state of the pulse than on anything else as a guide to the exact seat of the lesion.

DR. KILGOUR'S DIAGRAM OF MORBID SOUNDS OF THE HEART.



Aortic valve .... { Obstruction or contraction — Blowing sound supplants first sound.*
{ Patency with regurgitation — Diastolic.... — Blowing sound supplants second sound.*

Mitral valve .... { Patency with regurgitation — Systolic.... — Blowing sound supplants first sound.*
{ Obstruction or contraction (rare) — Blowing sound supplants second sound.*

Or,

Blowing in place of first sound is during a current of blood *from* a ventricle .... { Obstruction at aortic valve. Regurgitation at mitral valve.

Blowing in place of second sound is during a current of blood *into* a ventricle .... { Regurgitation at aortic valve. Obstruction at mitral valve.

Blowing in place of first sound { Obstruction at aortic valve — Blowing sound loudest at *base* of heart and carotids, and pulse commonly regular, often full and jerky.
{ Regurgitation at mitral valve — Blowing sound loudest at *apex* of heart, sometimes not present, and pulse commonly irregular, small, and weak.

Blowing in place of second sound { Regurgitation at aortic valve — Blowing sound loudest at *base* of heart and carotids, and pulse commonly regular, often full and jerky.
{ Obstruction at mitral valve (rare) — Blowing sound loudest at *apex* of heart, sometimes not present, and pulse commonly irregular, small, and weak.

Purring tremor *(fremissement)* is *felt* sometimes, in disease of the valves, by the hand laid over the heart.

Purring sound *(frottement)*, or creaking, or see-sawing sound, is heard in disease of the pericardium.

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* Similar sounds arise from disease of the corresponding valves of the right side; but disease of the left is twenty times more frequent than of the right.

When so much has been written on the treatment of the diseases of the heart, it may be something like going over a more than twice-told tale to offer any remarks on that subject. I shall, however, be brief, and the want of novelty will perhaps be atoned for by the fact, that I speak from the results of considerable experience, and that I offer these observations to the young practitioner chiefly.

If any one were to ask me "What can you do to cure disease of the heart?"—I would honestly answer at once, "Nothing." But if any one were to ask "What can you do to ward off the symptoms or feelings that too often become concomitants of that disease?"—I would say that, next to quietness of mind and body, and equable temperature, and moderate or rather low living, I know nothing equal to the use of small doses of colchicum wine, weak saline purges, and inserting a seton over the region of the heart. It will not do to trust to these when the severe sequelæ of heart disease—distressing dyspnœa, hæmoptysis, or dropsy—have more than once occurred; but I know no means equal to them in warding off these latter. I could mention several instances where this treatment has proved most beneficial. One case has been repeatedly under my notice. It was a female, a servant, past the middle period of life, who applied first for a slight attack of anasarca, arising from heart disease. The effusion was soon removed, and she was sent out of hospital with a seton in the side. She kept it open for some time; but thinking herself well, withdrew it. In a few days after she returned, begging that it might be re-opened,—which was done; and painful experience has more than once taught this woman, in the course of eight years, the necessity of keeping up a discharge from a seton, renewed when necessary, and aiding its influence by the occasional use of the above medicines.

It has often been doubted whether blood should be abstracted in cases of advanced heart disease in elderly persons, when labouring under a sudden paroxysm of violent dyspnœa, giving the feeling of almost immediate suffocation. No doubt, mustard poultices over the heart, and the immersion of the feet and legs in hot water holding some mustard in it, will give some relief; but no means will be found equal to abstracting a small quantity—from two to four ounces—of blood from the arm.

In the treatment of cardiac dropsy, every practitioner has his favourite remedy, taken from the class chiefly of diuretics or purgatives, or both, Of the former, I have found the following combination the most efficient. It pumps the patient out, so to speak, sometimes in a few hours: and it often will do so in repeated attacks of the anasarca.

> ℞. Infusi digitalis, ℥iv.; acetatis potassæ, ʒij.; spiritus ætheris nitrosi, ʒij.; aquæ cassiæ, ℥iss. Capiat cochleare magnum quartâ quâque horâ.

At last there comes an attack in which this and other diuretics cease to act, and we must then fall back on purgatives. Of the latter, unquestionably the most powerful is elaterium. But there surely must be a very great diversity in the strength of this medicine. Some practitioners, from the days of Sydenham, and long before him, downwards, appear to have given it in the dose of two grains, or even more; but

I have found a single pill, according to the following formula, generally very powerful:—

℞. Elaterii, gr. j.; extracti colocynthidis comp., ℈ijss.; extracti hyoscyami, gr. xij. M. Divide in pilulas xij. Capiat unam nocte maneque.

The great objection to the elaterium is the intense sickness, even in this small dose, produced by it. Do the large doses produce less sickness than the smaller? It may be so, but in the few instances in which I have tried large doses, the sickness was not less. Is there any mode by which this sickening property in this valuable medicine could be removed?

The combination of a bitter purgative with a saline one composed of the vegetable alkali and a vegetable acid, is in my experience much more efficient than any single purgative, or than a bitter with a salt formed of a mineral acid. The old compound powder of jalap is a well-known instance of a mixture of this kind, and is still one of our best purgatives in all dropsies where this class of medicines may be suitable. Ferriar used, as did also Home, a combination of half an ounce of the bitartrate of potass with two grains of gamboge. The infusion of senna with bitartrate of potass is also an old-fashioned and valuable remedy; but the insolubility of the salt is an impediment to the efficiency of this formula. The senna infusion with tartrate of potass, or with tartrate of potass and soda, is not liable to the same objection; and the advantage of the frequent use of this combination in cardiac disease having a tendency to dropsy, or in the dropsy itself attending that complaint, has been in my hands, and those of my brethren to whom I have recommended it, so unequivocal, that I can speak for it in the highest terms.

There comes a time in the treatment of this complaint when not only diuretics in all forms, but even purgatives, cease to remove or even to keep in check the anasarca. And this brings me to speak of another mode of treatment, which often proves palliative for a time,—viz., puncturing the lower extremities, and thereby draining off the fluid.

I do not, however, recommend it until all internal remedies have failed. I never have recourse to it till it has become, to use Mead's expression, "unica spes salutis;" and I do not forget the qualification he makes, "et ea dubia." Still I am decidedly of opinion that, whether as a palliative or as a hope of cure, it deserves more notice than now-a-days it seems to me to get.

There are two modes, as stated above, in which the opening for draining off the fluid may be made,—either a puncture with the lancet, or with a good stout sewing needle. From a single puncture by the latter, a quantity of water will sometimes run out so as to pass through the bed in a few hours, and require to be collected in vessels placed below it.

Heberden has evidently seen the danger attending this mode of treatment, though he does not say specifically in what it lies. He tells us that the wounds are difficult to be healed: but that we need not fear. He also says they are "periculosa." Now the danger consists in the inflammation of an erysipelatous character, that not unfrequently takes place, extending up the limb, sometimes bringing on *sphacelation*, and for the most part ending in the death of the patient. But if the medical attendant would be satisfied with one incision, and one, or at most two, punctures, in the proper place, erysipelas is not so apt to occur. The state of the skin—its low vitality during the existence of the dropsy under it—is singularly favourable for this form of inflammation; but a small clean cut, or one or two punctures,—and, if there be two, at a distance of not less than two inches from each other,—is not so likely to be followed by this as when, in the anxiety to get the water all off speedily, several have been made. The writers whom I have quoted all recommend fomentations to be applied to the wound; but I have often found the evaporating lotion very successful in keeping down any disposition in the part to erysipelas. Frequent ablution also, with tepid water, and the removal of any cloths that may be soaked with the serum that has run out, will serve to prevent, so far, any irritation from being set up in the skin.

When the operation, even with a needle, has to be often repeated, as is sometimes the case, for the ease of the patient, especially in cardiac dropsy, there is sometimes a degree of inflammatory action set up in the cellular tissue by which its interstices become obliterated, and the skin and it become quite closely adherent to the fascia below. I remember this occurring in a gentleman, who had been kept alive for some months by punctures, and who was at last dropsical everywhere, except below the knees, where the adherent and thickened integuments would not distend,—thus giving him, however, not a little pain. It has of late been recommended to have recourse to this treatment much earlier in the disease than was the custom with the old practitioners; and it has been said that the chances of success from it are much greater than when the patient's strength has been exhausted, and the vitality of the skin has been impaired by the long existence of a dropsical effusion. This reasoning is fair; but whilst admitting it, one practical point may be mentioned, which is, that the fluid does not flow so freely from the incisions as when the areolar tissue has become more open and less resilient by the long existent pressure of the effused serum. And little as the danger of erysipelatous attacks may be from puncture of the skin in the early stage of anasarca, still there are few that will subject the patient to this chance of danger until all the other more usual and often successful modes of treatment have been tried in vain.—*Monthly Journal of Med. Science, Sept.*, 1850, *p.* 193.

27.—CARDIAC DROPSY.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

(Reported by Mr. H. H. Salter, B.A.)

[In this case, the patient, Alfred Baylis, aged 30, was admitted on the 11th of November, suffering from palpitation, shortness of breath, and emaciation, occurring after an attack of hæmoptysis. On the occurrence of another severe attack of hæmoptysis, orthopnœa supervened. He also suffered under amaurosis—varying with the palpitation of the heart —the blood having become so poor that the impairment of the quality caused any slight deficiency in the quantity sent to an organ, from inefficient action of the heart, to be felt by it more readily than if the blood were in its normal state. Dr. Todd has also seen this state in connection with enlarged thyroid gland, and particularly in women. The only thing explaining the existence of this cardiac disease, was, that the patient, at the age of 19, suffered a slight attack of rheumatism.]

His state on admission into the hospital was as follows:—Great exsanguineousness; breathlessness, particularly orthopnœa. Heart's action natural and regular, with no preternatural impulse; pulse 84, small, regular. There was an unnatural extent of dulness over the region of the heart, and moreover the pulsations of the heart were visible and sensible in the region of the scrobiculus cordis; they were felt also in the cardiac region. In some cases the heart's action is felt only in the scrobiculus cordis, and beneath the left mamma; and in such there is no evidence of enlargement; for there may be, and probably is, some cause pressing the heart to the right side, so that the altered seat of the pulsations is due to change in the position of the entire organ. But when the pulsation of the heart is felt in both regions we have evidence of enlargement, and of its being, at least, the right ventricle that is enlarged; for while the left remains stationary in its normal situation, the right has passed over still further to the right side. Those of you who accompany me regularly through the wards of the hospital may notice, that in all cases of dyspnœa I make it a rule to place my hand over the scrobiculus cordis to feel if I can detect any cardiac pulsations there. My attention is particularly directed to this point in cases of long-standing asthma, and rarely in these cases do I fail to find it; never, indeed, if the asthma have produced emphysema, where there is not only displacement of the right ventricle by hypertrophy, but a pushing of the whole heart to the right side from the enlargement of the lungs.

We noticed also in this case a friction sound accompanying the systole and diastole, heard below and to the left of the nipple, and very circumscribed in extent. There was no bellows murmur either at base or apex; but we observed a phenomenon, connected in its indications with the beating of the heart, in the scrobiculus cordis,—viz., venous pulsation in the neck. This man has very large jugulars, and you may see them pulsating distinctly even at some distance from the bed. This phenomenon, as you know, is due to a reflux of blood at the systole from the ventricle through the auricle into the venous system. In consequence of the obstruction in the lungs the ventricle cannot completely empty itself, and so becoming distended the tricuspid valve allows some of the blood to regurgitate, and thus to escape through the auricle into the veins. The proper source of supply to these veins is from their distal extremity. If then you put your finger in the course of that supply,—that is, to the distal point where the pulsation is observed,—the vein would, if healthy, become empty; but it does not; it continues full, and its pulsations persist: hence the waves of blood which produce the phenomenon of pulsation must be propagated from the cardiac extremity of the vein. There are many cases of heart affection where there are no pulsations, but where the vein does not empty itself on the cardiac side of the point pressed. Now in such cases there is regurgitation, but not to a sufficient degree to cause the pulsation. In perfect health the vein will be quite empty below the point pressed: but it is possible that a slight enlargement may suffice to produce the opposite state; and it is very possible that, even in health, it may exist to a slight extent under the prolonged though temporary occurrence of some disturbing circumstances, such as great efforts, violent actions, &c. When the regurgitation exists in a great degree, you may see all the superficial veins of the neck and chest pulsating.

We found also in this case evidence of great pulmonary congestion, crepitus and rhonchus being heard over various parts of the chest; and there was abdominal dropsy, caused by obstructed circulation through the liver. On examining the urine we found it acid, of specific gravity 1015, and heat and nitric acid gave evidence of the existence of albumen. When the fresh urine was allowed to stand, a scanty and flocculent deposit was formed, which, under the microscope, was found to consist of blood-corpuscles, epithelium (chiefly renal,) a few pus globules, and a great many casts, for the most part granular and transparent, some containing cells loaded with oil globules; but these were not very abundant. Now this is a state of things which we should fancy our patient would not be likely long to survive; yet here we see him remaining very much in *statu quo*, if anything a little improved, until the present time, Jan. 16th, a space of two months.

Diagnosis.—Now can we, from these general symptoms, make a diagnosis without reference to the physical signs? I think we can form a diagnosis which shall be sufficient for all practical purposes; and I recommend you, as an important exercise, to endeavour to diagnose from symptoms before you have recourse to auscultation; because by so doing your attention will be directed to the disturbed *functions*, and you will thus receive important suggestions for treatment. Those who direct their attention exclusively to physical signs are apt to overlook these derangements of functions, and therefore miss some valuable indications of treatment corrective of them.

Now if I were to proceed on the data afforded by the *general* symptoms alone in this case, I should diagnose first, *dilatation of the right side of the heart:* the pulsation of the jugulars, and the beating of the heart in the scrobiculus cordis, both indicate this. The pulsation of the jugulars, if proved to be regurgitant, is conclusive evidence of dilatation of the right ventrical: no other condition of heart is capable of producing this phenomenon. But we must be careful to prove that the venous pulsation is regurgitant, as it may be due to other causes: It may depend on the impulse of the heart being transmitted through the capillaries; or it may depend on the contiguity of a large artery which communicates its pulsations to the adjacent vein; therefore we must be careful in proving its nature before we attach importance to its indications. Now by making pressure on the vein, we obtain sufficiently conclusive evidence as to the nature of the pulsation. If the vein empty itself on the cardiac side of the point pressed, and yet the pulsation continues in the empty portion of it, we know it arises, with the highest probability, from some contiguous artery; and this conclusion is rendered certain if the pulsation ceases on stopping the circulation in the artery. If, on making pressure, the pulsation continues only on the distal side of the point pressed, we may infer that the pulsation is due to the transmission of the pulse from the heart throughout the circulation, and especially if it be perfectly synchronous with the heart. But if the vein continues full between the point pressed and the heart, then we know, whether there be pulsation or not, that that vein must receive blood from its cardiac side, and this it can only do by regurgitation from the right side of the heart.

You are doubtless aware that a slight regurgitation always takes place from the auricles into the veins in the normal state, and that that regurgitation is synchronous with the auricular systole. But when the ventricle and auricle are dilated, the amount of regurgitation is increased in proportion to the degree of dilatation of the cavities: blood regurgitates from the ventricle into the auricle, and from the auricle into the veins.

The beating of the heart in the scrobiculus cordis is also indicative of dilatation of the right ventricle, because by its dilatation that cavity extends to the apex, and the apex thus enlarged extends to the scrobiculus. But, before we can affirm that the beating of the heart in the scrobiculus is due to the dilatation of the ventricle, we must be careful to ascertain that there is no displacement of the heart, which may be easily determined by seeing that its impulse is felt in the normal, *as well as* in the abnormal region.

So far, then, we may affirm, that there is dilatation of the right side of the heart. But such a lesion as dilatation does not arise spontaneously. There must be some dilating force, and that force is the backward pressure of the blood. When the muscular tissue of the heart is in a weak state, we can conceive that the walls of the ventricle may yield readily, without supposing any great increase in the ordinary pressure of the blood. But, in general, dilatation arises from some obstacle to the flow of blood from the ventricle; and in proportion to the resistance which that obstacle affords will be the backward pressure of the blood on the surface of the ventricle,—in other words, the amount of obstacle will give the amount of dilating force. In every case of dilatation of one of the heart's cavities, then, you must look for this dilating force,—this obstacle; and you will find it either at the orifice through which the cavity expels its blood, or in the arteries, or in the capillaries of some important organ, or in some defect in the valves or the orifices of the heart. Dilatation of the right cavities of the heart rarely arises from diseases of their own orifices; for obstructive disease of these orifices is amongst the rarest of cardiac lesions. Experience tell us that the obstructions which the right ventricle has to encounter are either from the circulation through the lungs, or from some derangement of the circulation through the left side of the heart.

And there is another cause which may produce dilatation as well as hypertrophy of the right ventricle, because it may produce the same change in all the cavities of the heart; that is, extensive or universal adhesion of the opposed surfaces of the pericardium. How it happens that obliteration of the pericardial sac can produce these changes, I do not attempt to explain; but that it does so there can be no doubt.

Let us now inquire what may cause a dilated right ventricle in this case. I have said that the most common causes of dilatation of the right ventricle are obstructed pulmonary circulation, or obstructed circulation through the left side of the heart. The most common causes of obstructed pulmonary circulation are the asthmatic state, or that which is a frequent consequence of the asthmatic state—emphysema of the lungs. We have no indications in this case of either of these conditions. The history of the patient shows the absence of the one, and the physical signs demonstrate the absence of the other. Is there, then, any disease of the left side of the heart sufficient to create this dilatation?

When I saw the patient first I hazarded a guess, from the character of the pulse, that there was *mitral* disease at the foundation of his malady. The peculiarity of pulse which suggested this notion was, that it was small, produced by a small stream, apparently inadequate to the size of the artery, and not proportioned to the action of the heart. Such a state of pulse does frequently co-exist with mitral disease; but so various are the conditions capable of producing a small feeble pulse, that a diagnosis based upon that sign only must necessarily be only a guess. Now such mitral disease might be either *regurgitant or obstructive*—either of the two would affect the right side of the heart. And how?—By affording an obstacle to the return of the blood from the lungs. Whether the blood is unable to advance in consequence of a *contracted* mitral orifice, or whether it flows back in large quantity into the auricle, it is evident that an impediment is offered to its passage from auricle to ventricle, and, therefore, to its return from the lungs to the auricle; and when this has continued for some time, the right ventricle being exposed to a degree of pressure greater than customary, yields, and gradually expands, till at length it assumes a permanently dilated state.

Now, is the disease here regurgitant or obstructive? The dyspnœa favours the idea that it is obstructive, because there is no form of heart disease in which the dyspnœa is so great as in obstruction at the mitral valve. Very often, when the disease is of this nature, sudden and terrible dyspnœa is the first evidence of its existence; the patient is suddenly awoke out of sleep by a breathlessness that seems as if it would kill him: he starts up in bed and gasps for breath till the paroxysm gradually subsides. Such a first attack is generally the precursor of similar ones, increasing in severity and frequency, only kept off by constant watching and the upright position, till at length the dyspnœa becomes constant, and the case terminates with pulmonary apoplexy.

But in the absence of this disease of the mitral valves, is there any other way of explaining the occurrence of the dilatation of the right cavities of the heart? The only explanation I can offer is that there is hypertrophy and dilatation of the left cavities connected with obstruction in some part of the systemic circulation, in the aorta or in the renal circulation, or dependent on adherent pericardium, and that this state of the left cavities obstructs the circulation through the right cavities, and causes dilatation and hypertrophy of them.

So far, then, for the indications afforded by the general symptoms: let us now turn to—

The physical signs.—A careful observation of the physical signs will serve to confirm or correct our inferences from the symptoms, and help us to adopt a more decided diagnosis. The physical signs themselves were these: extensive dulness over the præcordial region, indicating enlargement of the heart; the first and second sounds were found to be essentially healthy, and unaccompanied with bellows murmur, as well at the base as at the apex. The absence of bruit at the base shows that there is *no* aortic disease; also its non-existence at the apex shows that there is no *regurgitant* mitral disease; for if there were, we should inevitably hear a systolic bellows murmur in this situation—*i. e.*, unless the heart's action were very weak: it is only in a state of great feebleness of the ventricular contraction that there would be no bellows sound where there is mitral regurgitation; but with our patient the heart's action is not at all feeble. Now, is there any diastolic bellows sound to indicate obstruction at the mitral orifice? We find none. Are we, then, justified in denying the disease of the mitral valve? Certainly we are not, for those cases of unquestionable obstructive mitral disease in which a diastolic murmur is heard are comparatively few: there must be a certain amount of force in the auricle for its production, and unless this exists the sound will not be heard. The value of this evidence, therefore, is merely negative: it does not prove that there is no obstruction at the mitral orifice, but only that the impulse given to the blood by the auricle is not sufficient to elicit a sound from the existing obstruction. So far, then, we receive really no aid as to determining the question of the existence of obstructive mitral disease from examining the sounds of the heart. We know, however, that there is no mitral regurgitation; and, as to the question of mitral obstruction, we have no further evidence than what the symptoms give us.

This evidence is undoubted as to the existence of obstruction; but it is not sufficient to determine the exact seat or nature of the obstruction. By the aid of auscultation we can determine that it is not mitral regurgitation: it may, however, arise from contraction of the mitral orifice, or from that general obstruction which the left heart has to encounter in the distal part of the systemic circulation.

Taking it for granted, then, that the circulation through the left side of the heart is obstructed, it is easy to explain all the other symptoms. The obstruction through the left side of the heart throws the blood back on the left auricle, and thence on the lungs, giving rise to the state of extreme pulmonary congestion in which we find our patient, and the consequent hæmoptysis. The obstruction at the lungs is felt by the right ventricle, which, in its efforts to overcome the opposing obstacle, becomes dilated and hypertrophied: the right auricle encounters corresponding difficulties to those which oppose the action of the ventricle, and it becomes similarly dilated and hypertrophied; and, in consequence of the impediment which it has to encounter, the blood is thrown back upon and delayed in the venous system, and regurgitant venous pulsation and dropsy are the result. Thus we see all the symptoms fall in a retrograde succession, as the consequences of the obstructive force beginning at the left side of the heart, or in some part of the systemic circulation.

A feature of this case which practically is of most interest to us is the *dropsy,* because this is the symptom which generally and justly excites the most alarm in the minds of patients and their friends. I have already mentioned that the characteristic of cardiac dropsy is this, that it always takes place in the most dependent parts: it does not begin, as we have seen other forms of dropsy, in the face and upper extremities. An aneurism may cause dropsy of the upper parts of the body from pressure on the descending cava or some of its large tributary veins, but pure cardiac dropsy never comes on thus: the very fact of its not existing in the upper parts of the body indicates its cardiac nature. We find it apt to vary in amount according to position; the erect or sitting posture favours its accumulation, and hence we find it in increased quantity towards the end of the day, and disappearing or diminishing in the morning from the recumbent position during the night. Still, it *will* appear in the upper extremities, but always late, and often more on one side than another, being most abundant on that side which is the most dependent. This we have seen in our patient: his left hand was the first affected; but lately we find he has been lying very much on his right side, and the consequence is that the dropsy has left his left hand, and accumulated in his right. We could not have a better proof than this of the purely mechanical character of the effusion.

Now what is the immediate cause of this dropsy? We have seen that the obstructing force extends from the left to the right side of the heart, and by its influence in the latter situation a considerable portion of the blood returning from the general circulation is pumped back into the veins at every systole: this obstructs the flow of venous blood throughout the body: the circulation is consequently impeded in the capillaries: these vessels become loaded, and the only relief they can obtain is by the filtration of the serum of the blood, or the liquor sanguinis, into the areolar tissue. If any part is dependent, a greater force is needed to send the blood through its capillaries, because the venous blood has to ascend against gravity: a greater accumulation is apt to take place both in the capillaries and veins; and, therefore, such part becomes affected with a greater amount of dropsy.

The access of dropsy is always a serious symptom, but not necessarily a fatal one. It may come on early or late. In some cases of purely cardiac disease in young persons, not complicated with either kidney or liver disease, dropsy may come on early, from which the patient may recover completely, and life may be prolonged for some time even although the cardiac disease continue. Suppose a patient has had rheumatic endocarditis, and that the impairment of the mitral valve gives rise to regurgitation, the effect would be to obstruct the circulation first at the left, and then at the right side of the heart. Now the right cavities feel the obstruction at first much more than they would do afterwards: the left auricle, being slow to dilate, has not yet adapted itself to the deranged condition, and the whole force of the obstruction is thrown back on the lungs and on the right side, the cavities of which dilate easily, and you get rapid dropsy. In the meantime the left auricle dilates, the circulation through the lungs becomes less obstructed: there is, therefore, less obstruction in the right side of the heart: it gradually recovers itself, and the dropsy diminishes.

In all cases what seems to be necessary to the production of the dropsy is the *dilatation of the right side of the heart.* I think my friend Dr. Blackiston, of St. Leonard's, was the first to point out clearly the importance of this condition. Dr. Blackiston seems to lay it down as a rule that venous regurgitation always precedes the dropsy. But dilatation may exist without regurgitation being manifest in the external veins: at least, I think that the regurgitation need not exist to such a degree as to produce pulsation. I am now attending a case in private practice, in which there is dropsy and undoubted dilatation of the right side of the heart, but no venous pulsation.

But there are certain other circumstances in this case, in addition to the condition of the heart, that predispose to dropsy: these are, a state of the blood in which the water is in large proportion, and its solid ingredients defective, and *renal disease.* When the blood is in such a condition as to favour dropsy, the other causes will act much more speedily than when such is not the case; and, no doubt, in the present instance, the dropsy came on much earlier in consequence of the impoverished state of the blood, occasioned by the repeated attacks of hæmoptysis which our patient had suffered.

Reviewing, then, the whole case, we may sum up thus: that the primary cardiac affection consisted in a rheumatic endocarditis, which narrowed the aperture of communication between the auricle and ventricle, and thus obstructed the passage of the blood into the ventricle, throwing it back upon the lungs, causing pulmonary hæmorrhage, and dilatation and hypertrophy of the right cavities; or, in a rheumatic pericarditis, causing an extensive adhesion between the heart and the pericardium, and so far impeding the heart's play as to give rise to hypertrophy with dilatation of its cavities. Or that, from some obstruction to the circulation, either in the large arteries, or in some more distant part of the circulation (in the kidney, for instance, from the chronic disease in that organ), the left heart has suffered dilatation and some hypertrophy; and that, in course of time, the right heart has, in consequence of the obstructed circulation in the left side, become hypertrophied and dilated likewise. On the former supposition, the primary disease would be in the heart itself; on the latter, it would be in the kidneys, or some other part of the systemic circulation.

I have already alluded to the amaurosis with which our patient is affected. This affection most probably results from an anæmic state of the retina. Last summer we had in the hospital a remarkable case, which illustrated how loss of blood produced amaurosis. The man had suffered from violent hæmatemesis: after the last attack he completely lost his sight, and he remained quite blind afterwards, notwithstanding a variety of treatment to which he was subjected. In a case of this kind the nutrition of the retina is permanently damaged by the altered quality or quantity of the blood after such severe hæmorrhage, and it bears an analogy to those cases in which delirium or epilepsy follow excessive losses of blood, or extreme states of anæmia.

Treatment.—In the treatment of cases of this description your attention should be principally directed to the relief of the urgent symptoms, and to strengthen the power of the heart. For the relief of the dropsy a great deal may be done by attention to the position of the patient, and by the administration of diuretics. With the view of supporting his power we gave our patient Baylis tonics and iron; and, finding the kidneys acting imperfectly, we gave him digitalis. But in such cases it is desirable to be careful in administering this medicine, and it is a good plan to combine it with some tonic. I frequently combine it with ammonia, or give the tincture of digitalis with the tincture of muriate of iron. However, I cannot say that any great benefit resulted in the present case, either from the digitalis or the iron: that treatment was cut short by the supervention of the pericarditis. This, however, was subdued by very simple means—counter-irritation and copious discharge by the blister.

There was much difficulty in getting the kidneys to act, and the greatest benefit was derived from the bitartrate of potass, either alone or in combination with the pulvis jalapæ, or the compound powder of jalap. We also gave him lemon-juice, and found it acted freely on the kidneys for some time. The dose of lemon-juice was from half an ounce several times a day.

Elaterium was also given; but without any decided benefit. What, however, has served best to keep down the dropsy of the legs is the acupuncture: the quantity of water that has run away from him, and the relief that he has obtained from it, is surprising. In practising acupuncture in dropsical cases it is important that you should make the punctures at a considerable distance from each other, for each puncture is apt to become the centre of an erysipelatous inflammation, which spreads the more readily when the punctures are too near each other. Indeed, however you make the punctures, the difficulty which you have to encounter arises from this erysipelatous inflammation, which almost invariably follows sooner or later. There is another method of relieving anasarcous legs, lately proposed by Lombard, of Geneva. Instead of pricking the legs at various points with the needle, he advises to make a single incision above the malleolus, or in some other convenient position (not too near a vein or artery), cutting through the skin and subcutaneous tissue down to the fascia. The length of the incision may be from half an inch to an inch, and the rapid flow of water that these incisions admit of is furthur encouraged by sitting the patient on the side of the bed for a certain time during the day with his feet placed in a tub. The patient has by this means the treble advantage of increased rapidity of the flux of the fluid, the prevention of that sloppy and soaking condition which inevitably ensues when the acupunctures are running all day, and the ease to the breath that results from the erect position. There is less danger of sloughing by this process, as a single incision is sufficient for each leg. Unhappily we can do nothing to restore the impaired action of the heart, but we may do much to ameliorate the condition that that impairment entails. The principal indications are to support the patient's strength, to keep open the emunctories, and to adopt all the means in our power to get rid of the superfluous water.—*Med. Gazette, Sept.* 20, 1850, *p.* 480.

28.—ON DISEASES OF THE HEART.

By Dr. O'Bryen Bellingham.

[The abnormal sounds of the heart were first termed murmurs by Dr. Forbes, but as they are in every instance the result of friction, they would be more completely classified according to their seat, those which have their seat external to the heart being termed pericarditic or exocardial, and those within the heart endocarditic or endocardial. The exocardial murmurs, however, are more particularly entitled to the term friction sounds, as they take place between two solids, for in all the others one of the agents producing the sound is a liquid. As in a state of health the opposed serous surfaces of the pericardium glide smoothly over each other without any perceptible sound, so, if by inflammation these surfaces should be rendered rough by deposition of lymph, the friction then developes the sound, and even sufficiently strong to communicate a distinct vibratory sensation to the hand, the sound varying in intensity in proportion to the action of the heart, and the quantity of the deposit. The friction sounds of pericarditis may be included under the following heads:—1st, A slight friction sound. 2nd, A rasping sound. 3rd, The sound resembles "the creaking of new leather," as originally compared by M. Collin.]

Conclusions respecting the pericaditic friction-sounds.—1. That they are almost limited to cases of inflammation of the pericardium; that they are pathognomic of one form of this disease, and constitute the most valuable diagnostic signs of it.

2. That they have all, more or less, a friction or attrition character.

3. That they are almost always double, and are usually better marked during the period of the ventricular systole than during the diastole.

4. That they appear to be superficial and near, and are seldom audible much beyond the limits of the præcordial region.

5. That they do not, like the murmurs next to be described, replace the ordinary sounds of the heart, but are heard independent of them.

6. That their duration is usually short; frequently ceasing entirely after having been heard for a few days, and not unfrequently changing their character within the period that they are audible.

7. That they are frequently accompanied by a peculiar vibration or frémissement sensible to the hand laid upon the parietes.

8. That a bruit de soufflet at one of the orifices of the left side of the heart, the result of endocarditis, not unfrequently coincides with these sounds.

Endocardial or valvular murmurs.—The abnormal sounds which have their seat at the orifices of the ventricles, and which replace or accompany the normal sounds of the heart, constitute the most valuable signs of disease of the valves or orifices of the left side of the organ. They present several varieties in different cases,—being sometimes blowing, when they constitute the varieties of the bruit de soufflet or bellows murmur; sometimes rough and harsh, resembling the familiar sounds of sawing, rasping, or filing; and at other times having a whistling or musical character, constituting the varieties of the bruit de soufflet musicale, or musical murmur.

Bruit de soufflet.—The bruit de soufflet or bellows-murmur, as the name denotes, has a blowing character, and is by many degrees the most frequent abnormal sound heard in diseased states of the valves or orifices of the heart. It was discovered and named so, by Laënnec, from its similarity to this well-known sound, to which it often bears a most ridiculous resemblance. Bruit de soufflet does not always, however, present the same character: sometimes it is perfectly smooth and soft; at other times it is loud and rough; sometimes it is a mere whiff: at others it is prolonged and lengthened out. It may accompany or replace either the first or the second sound of the heart, more frequently the first; or it may pass into a sawing, rasping, or musical murmur. In the vast majority of cases of disease, it is limited to the left side of the heart.

Circumstances under which bruit de soufflet is heard.—Bruit de soufflet is heard in various and different lesions of the valves and orifices of the heart; it is not, however, limited to diseased states of this organ, but becomes audible under a variety of other circumstances, as we shall presently see. Thus, it is heard when there is any obstacle to the free passage of the blood through the orifices of the left side of the heart, or when the valves imperfectly perform their functions, and permit regurgitation. It is heard in aneurism of the left ventricle and in congenital malformations of the heart, where a communication exists between the ventricles. It is heard in cases where the lining membrane of the arch of the aorta is diseased, and in aneurism of all the large arteries. It is heard in aneurismal varix, where a communication exists between an artery and a vein, in varicose aneurism where a small sac exists between them, and in the pulsating form of aneurism by anastomosis. It is frequently heard in cases where no disease of any kind exists in the heart or arterial system, but where the quality of the blood is altered; where this fluid has become more watery and less viscid than natural. Finally, it can be developed at any time in the heart and arteries by suddenly abstracting a large quantity of blood, as was first pointed out by Dr. Marshall Hall; or in a large artery by making pressure upon it so as to diminish its calibre.

Bruit de soufflet may be developed in the cavity of the chest by the growth of a tumour of any kind, which comes to press upon the aorta or its branches, or upon the pulmonary artery; or by malformation or deformity of the chest, owing to disease of its bony parietes, by which the heart's movements are impeded or the large vessels are compressed. Bruit de soufflet is said to have sometimes been heard for the first time, a short period previous to death, in the advanced stage of pulmonary or other diseases: M. Bouillaud refers it then to the formation of fibrinous concretions in the cavities of the heart, which interfere with the action of the valves, or obstruct the orifices. It is temporarily heard in violent palpitation in hysterical subjects, or in attacks of palpitation in cases where the left ventricle is hypertrophied, and its cavity dilated: here it disappears as the palpitation subsides. Bruit de soufflet, in a very marked form, is likewise heard from the fourth month to the end of pregnancy: here its site is usually the iliac fossa, sometimes on both sides, more frequently only on one side.

Dr. Graves has noticed the remarkable fact, that a bruit de soufflet is sometimes audible, in the second stage of pneumonia, over the affected lung, which disappears as the symptoms of the inflammation subside: it is not perceptible in the subclavian or carotid arteries of the same side, and probably, when present, depends upon pressure on the branches of the pulmonary artery. Dr. Latham has called attention to the circumstance that a gentle bruit de soufflet, which coincides with the ventricular systole, is heard occasionally in phthisis: it is not perceived in the præcordial region, but in a circumscribed space above it and on the left side M Zehetmayer has confirmed the correctness of Dr. Latham's observation, and would appear to have noticed this murmur previous to the publication of Dr. Latham's work. He says, "I have repeatedly heard, in phthisical persons, in the second intercostal space, a decided bellows-murmur, instead of the first sound of the pulmonary artery; and in a place where there was no doubt that tubercular infiltration was present. No alteration in the vessel could be detected on post-mortem examination." Dr. Latham supposes that this murmur may have its cause in the pressure exercised by the diseased lung upon the pulmonary artery, and its first branches. In both the latter cases, the condensed lung, having become a better conductor of sound, conveys a murmur to the ear which would probably be inaudible if the lungs were healthy. M. Gendrin has called attention to the circumstance that, in the cold stage of intermittent fever, a bruit de soufflet, analogous to that heard in cases of chlorosis, is audible on auscultation of the heart. As individuals who been the subjects of ague for any length of time are, however, in general more or less anæmic, the murmur is probably due to this cause.

Conditions under which bruit de soufflet is developed.—Whenever bruit de soufflet is heard in the heart, it arises either from contraction or some other diseased state of the orifices or valves of the heart, which impedes the free passage of the blood; or from a condition of the valves which prevents them from closing the orifice, and permits regurgitation; or from the blood being impelled with increased force and velocity through the aortic orifice, or through an abnormal orifice, as in aneurism of the left ventricle, and congenital malformations of the heart; or from some alteration in the quality of the blood itself, or in its quantity; or from the formation of fibrinous or other concretions in the cavities of the heart, which interrupt the play of the valves, or impede the orifices; or where morbid growths are developed in the cavity of the chest, which compress or displace the heart; or where the bony walls of the thorax are deformed, and the cavity is much narrowed.

Whenever bruit de soufflet is audible in the arteries, it arises either from roughness of the lining membrane of the vessel; or from the calibre of the artery being diminished, owing to pressure upon the part; or from an altered condition of the blood; or from an aneurismal sac springing from a large artery; or where an abnormal communication exists between a large artery and a vein; or where an aneurismal sac is seated between an artery and a vein, through which the blood passes from one to the other.

[The increased friction between the blood and the parts along or through which it passes is the physical agent producing the bruit de soufflet, and this appears to be amply sufficient not only to develope murmurs, but to convert the normal sounds of the heart into them. Thus—]

1. When the orifice of the aorta is contracted, or its valves are diseased, so as to impede the direct passage of the blood, there must necessarily be increase of friction; and the normal first sound of the heart will be converted into a murmur.

2. When the mitral valve is diseased, and permits a refluent current of blood into the left auricle at each ventricular systole, a murmur will be developed, which, from its louder tone, will obscure or render inaudible the first sound of the heart.

3. When the similunar valves of the aorta imperfectly close the orifice, the blood must regurgitate from the aorta into the left ventricle at each ventricular diastole; and that this occurs with sufficient force to generate a murmur, which obscures or renders inaudible the normal second sound of the heart, is well known.

4. When the left auriculo-ventricular orifice becomes dilated, and the valve cannot, from this cause, fulfil its function perfectly, regurgitation of the blood occurs at each ventricular systole, and a bruit de soufflet is developed, which is usually sufficiently strong to obscure the normal first sound of the heart.

5. When the blood is propelled with greatly augmented force, and with increased velocity, through the aortic orifice, the amount of friction between the blood and the parietes of this orifice must be much increased; and if it is sufficiently strong to generate a murmur, this will have the characters of bruit de soufflet, and will take the place of the normal first sound of the heart.

6. When the lining membrane of the ascending portion of the arch of the aorta loses its natural smoothness and polish, the friction between the blood and the part along which it passes must be increased; and, if if the heart's action is strong, a murmur will be developed, which will be synchronous with the first sound of the heart, and will often be audible in the large vessels which come off from the arch of the aorta.

7. When the viscidity of the blood is diminished, and its watery parts are increased, a greater amount of friction will take place between this fluid and the orifice of the aorta, in its passage out of the ventricle, as well as in the large arteries, which is usually quite sufficient to convert the normal first sound of the heart into a murmur.

8. When the blood is propelled into an abnormal cavity, or through a preternatural orifice, and the action of the left ventricle is strong, the friction will be sufficient to generate a murmur which will obscure the first sound of the heart.

9. When the large arteries in the cavity of the thorax are compressed by the growth of a tumour within, or by deformity of the bony cavities from disease without, the diminution of their calibre is often sufficient to generate a murmur, which will have the character of bruit de soufflet, and will be heard at the period of the ventricular systole.

10. When lymph is deposited about the valves or orifices of the heart in sufficient quantity to prevent the action of the valves, or to obstruct the passage of the blood, there will necessarily be increased friction, usually sufficient to develope a murmur which may replace either the first or the second sound of the heart, or both.

11. When the fibrin separates, in the cavities of the heart, from the other constituents of the blood, and obstructs the orifices or impedes the action of the valves, if the action of the heart was strong sufficient friction would be produced to develope a murmur; but as this scarcely ever occurs, except during the last few hours or days of life, the current of blood is generally too feeble to develope a murmur.

12. The bruit de soufflet heard after the fourth month of pregnancy, which is not audible until the uterus has risen out of the pelvis, and which is generally known under the name of the "placental soufflet," appears to have its cause in the pressure exercised by the enlarged uterus upon the iliac arteries. Because it always accompanies the ventricular systole, it is audible usually only in one iliac fossa: and an exactly similar murmur has been heard by Dr. Montgomery in cases where the uterus itself was enlarged from disease, or where an abdominal tumour compressed the aorta. In addition, M. Bouillaud has found that, by changing the position of the patient, the site of the murmur is altered; and Dr. Cowan observes that the bruit de soufflet in these cases "can be detected in the femoral arteries immediately below the arch," and that "the sound may, by change of position, be transferred from one femoral artery to the other, always corresponding with the side of the uterine murmur."

In the majority of the foregoing examples the bruit de soufflet is heard at the period of the ventricular systole, and replaces or accompanies the first sound of the heart: the force with which the blood is propelled by the left ventricle being much greater than that with which it enters it, the friction between the blood and the parts along or through which it passes must be much more considerable in the former than the latter. In the former it is generally sufficient to generate a murmur, in the latter it only occasionally does so; and, when a murmur is developed, it has a different character. For instance, when the mitral valve or orifice is diseased, so as to permit regurgitation, a bruit de soufflet will be heard at the period of the ventricular systole, which quite obscures the normal first sound of the heart; when the aortic valves permit regurgitation, a bruit de soufflet is audible at the period of the ventricular diastole, which likewise obscures the normal second sound of the heart. But the two murmurs are very different: that which accompanies mitral regurgitation is loud, strong, and blowing; that which accompanies aortic regurgitation is soft, feeble, and whispering, because the force with which the blood is propelled out of the left ventricle is so much greater than that with which it enters the same ventricle.

Two or more of the conditions described above are not unfrequently combined in the same subject; and we may have a murmur taking the place of, or obscuring both sounds of the heart; or the bruit de soufflet may pass into a sawing, rasping, or musical murmur; or, finally, the valvular disease may be complicated with an anæmic state of the system, by which the difficulty of the diagnosis is sometimes considerably increased.

Bruit de soufflet at the right side of the heart.—The foregoing remarks have reference more particularly, to the bruit de soufflet developed at the right side of the heart: in fact, this abnormal sound is very rare at the right side of the organ, because the force with which the blood enters or is expelled from the right ventricle is generally too feeble to generate a murmur; while valvular disease at this side of the heart is uncommon. Dr. Clendinning has shown, from statistics in 100 cases, that valvular disease is sixteen times more frequent at the left than the right side of the heart. When we consider, too, that in cases of anæmia the murmur is limited to the left side, and that murmurs from regurgitation are never, or scarcely ever, produced at the right side of the heart, we can understand why bruit de soufflet should be so rare at this side. For instance, mitral regurgitation is one of the most frequent causes of bruit de soufflet at the left side of the heart: on the other hand, tricuspid regurgitation is even more frequent; but it is not accompanied by any morbid sound, and is usually recognized by another sign,—viz., jugular pulsation. Again, aortic regurgitation is a frequent cause of a murmur at the left side of the heart, but a very uncommon cause of a murmur at the pulmonary orifice. "I believe (Dr. Hope observes) diastolic murmur of the pulmonic valves to be exceedingly rare from disease of the valves themselves, as I have never met with a case, or been able to find one recorded." "From a rude numerical calculation (he adds) deduced from the cases that I have seen, I should think that there would be at least thirty chances to one against a murmur connected with the semilunar valves being seated in the pulmonic set."

Conclusions from the presence of bruit de soufflet.—The value of bruit de soufflet as a sign of disease of the valves or orifices of the heart, depends—

1st. Upon the period of the heart's action at which it occurs.
2d. Upon the situation at which it is best marked.
3rd. Upon its character or tone.
4th. Upon its being constantly present or not.

In organic disease of the valves and orifices of the heart, bruit de soufflet is in a great measure limited to the left side of the organ, and has its seat either at the aortic or mitral orifice. When it replaces the first sound of the heart, it depends, in almost all cases, either upon obstructive disease of the aortic orifice, or regurgitant disease of the mitral orifice. When it occurs at the period of the ventricular diastole, and of the second sound of the heart, it almost always has its cause in a state of the aortic valves permitting regurgitation. When bruit de soufflet accompanies both the systole and the diastole of the ventricles, it may have its cause either in obstructive disease of the aortic orifice, combined with a state of its valves permitting regurgitation; or in the mitral and aortic valves, both permitting regurgitation.

The first sound of the heart is not unfrequently replaced by bruit de soufflet when no disease of the valves or orifices exists; but a murmur is never heard at the period of the second sound of the heart, unless in cases of disease. A murmur at the right side of the heart is, as has already been observed, extremely rare; and, when it is heard, it will be found to depend more frequently upon disease of other parts than of the valves or orifices of the right ventricle.

[With regard to the so-called sawing, filing, and grating valvular murmurs, Dr. Bellingham remarks:]

It is a common opinion that the harsh grating valvular murmurs are the result of osseous or calcareous degeneration of the valve at which they have their seat, and that they always indicate a more advanced state of disease of the valves, or a greater amount of contraction of the orifice than bruit de soufflet. This was the opinion of Laënnec, and this appears to be the view taken by M. Bouillaud; it is far, however, from being correct: these sounds are heard in cases where the valves have undergone neither calcareous nor osseous degeneration; and a very advanced state of disease of a valve is often indicated simply by bruit de soufflet, while, in the most advanced stage of all, it may disappear, and no murmur of any kind be audible. This is sometimes witnessed at the mitral orifice, when the contraction becomes so extreme as not to permit a sufficient current of refluent blood through it to develope a murmur.

Although these harsh grating sounds do not necessarily indicate either osseous or cartilaginous degeneration of the valves, and are often not heard in cases where valvular disease is much advanced; yet, when they are well marked, we may conclude that the degree of friction between the blood and the parietes of the orifice at which they are developed, is greater than where simply bruit de soufflet is heard. In the majority of cases this is owing to hypertrophy, or hypertrophy with some dilatation of the ventricle complicating the valvular disease, by which the contractile power of the ventricle is increased, and the blood is transmitted with augmented force through the diseased orifice. Hence the rough valvular murmurs are limited in a great measure to the period of the ventricular systole and the first sound of the heart, and are not heard in inorganic affections of the organ, in which bruit de soufflet is so common.

In almost every instance the rough grating murmurs are preceded by bruit de soufflet; the one insensibly passes into the other; and the former are to be regarded as merely more intense degrees of the latter. For instance, when, owing to the disease of the semilunar valves of the aorta, or of the aortic orifice itself, the passage outwards of the blood is obstructed, a bruit de soufflet will be developed: if the impediment increases, owing to increase of the obstruction,—and if, as usually occurs, the parietes of the left ventricle become hypertrophied,—the bruit de soufflet will pass into a sawing, filing, or rasping murmur. On the other hand, when the aortic valves merely permit regurgitation, no increase of the disease will convert the bruit de soufflet into a rough or grating murmur,—because the force with which the current of blood enters the ventricle is never sufficiently great to develope a harsher sound than bruit de soufflet. The rough grating valvular murmurs are consequently limited to the period of the ventricular systole; they replace only the first sound of the heart, and are, in a great measure, limited to disease of the aortic orifice or its valves.

Conclusions from the presence of a Rough Valvular Murmur.—1. The sawing, filing, grating and valvular murmurs are to be regarded as nothing more than bruit de soufflet exaggerated.

2. They are limited to the period of the first sound of the heart, and accompany only the systole of the left ventricle.

3. They are only heard in cases of organic disease of the valves or orifices.

4. The diseased state in which they are most generally audible is narrowing of the aortic orifice.

5. In almost every instance, hypertrophy, with dilatation of the left ventricle, accompanies the valvular disease.

6. The sawing, filing, and grating valvular murmurs do not necessarily indicate either osseous, calcareous, or cartilaginous degeneration of the valves or orifices.

The abnormal sound heard most frequently in the arteries is bruit de soufflet. In a state of health, the blood, in its passage through the large arteries, causes such slight friction that very little sound is produced; this is short, single, and repeated at equal intervals, from sixty to eighty times in a minute, and is synchronous with the ventricular systole, and with the pulse. If slight pressure be made upon the vessel with the finger, or with the stethoscope, this sound increases in intensity; and if the pressure be increased, it is converted into a murmur having the character of bruit de soufflet. Increase of friction between the blood and the lining membrane of a healthy vessel, produced by merely diminishing the channel through which the current flows, is therefore sufficient to produce a murmur in a large artery.

Again, one of the most important properties of the blood is its viscidity. In some morbid conditions of the system this property is impaired; the blood becomes impoverished, attenuated, and watery; the red globules diminish; serum superabounds; and, as a natural consequence, the viscidity of this fluid is less than natural. In other cases the actual amount of blood in the system is diminished, as after profuse hæmorrhage, or when venesection has been carried to an extreme degree. In both these cases, the amount of friction between the blood and the lining membrane of the arteries must necessarily be greater than natural; and this increased friction is capable of developing a murmur analogous to the bruit de soufflet produced by pressure upon a healthy vessel. In order that the murmur may be well marked, it is necessary that a certain degree of velocity of the current above the natural standard should occur in addition.

The arteries in which bruit de soufflet is most commonly heard are the aorta, the carotids and subclavians, the iliac and femoral arteries. It may be heard in any one, or in all these vessels, when the blood is propelled with increased force and velocity through them; or when the calibre of the artery is diminished by pressure from without; or when the coats of the arteries have lost their natural and healthy state of tension; or when the blood is more or less attenuated, owing to its watery parts being increased, and the red globules diminished,—the result of profuse hemorrhage, or of a chlorotic state of the system. The latter are frequently accompanied by the abnormal sounds (presently to be described) which have their seat in the veins; in these, also, the action of the heart is more rapid than natural; and the arterial bruit de soufflet is sometimes accompanied by a murmur at the aortic orifice, depending upon the same cause.

The bruit de soufflet, which has its seat in the large arteries, has been mistaken for a valvular murmur, particularly when it has its seat in the arch of the aorta, or in the carotid and subclavian arteries. It may almost always be distinguished,—

1. By its character, which, as Dr. Hope observes, is "usually a mere whiff."

2. By its never being heard at the period of the ventricular diastole and the second sound of the heart.

3. By its being usually audible over several arteries at the same time.

4. By the facility with which the murmur is altered by varying the amount of pressure of the stethoscope, by which it is converted into a hissing, or harsh murmur.

5. By its not being constantly present: by its subsiding occasionally when the circulation becomes tranquil, and returning when palpitation ensues.

6. By the general signs of anæmia being present.

7. By the absence of the physical or general signs of organic disease of the heart.

8. By its entirely disappearing under treatment calculated to relieve the anæmic state of the system.—*Medical Gazette, Sept.* 6, 1850, *p.* 397.

29.—SPECIAL RULES TO BE FOLLOWED DURING AUSCULTATION OF THE CIRCULATORY ORGANS.

By Professor Bennett, Edinburgh.

[In a clinical lecture, this able physician gives the following special rules for auscultation of the circulatory organs:]

1. In listening, says Dr. Bennett, to the sounds produced by the action of the heart and arteries, we should pay attention,—1st, to the impulse; 2nd, the character and rhythm of the sounds; 3rd, the place where they are heard loudest, and the direction in which they are propagated.

You should commence the examination by feeling for the spot where the apex of the heart beats against the walls of the chest, which will enable you to judge of the impulse. This ascertained, place your stethoscope immediately over it and listen to the sounds. Then place the instrument above, and a little to the inside of, the nipple, near the margin of the sternum, and listen to the sounds there. In the one situation you will hear the first or systolic sound, in the other the second or diastolic sound loudest.

3. If anything different from the normal condition be discovered in either one or the other position, or in both, they should be again carefully examined, and by moving the stethoscope below and round the apex of the heart, or above, in the course of the aortic arch or carotids, on the right and left side, &c., &c., it should be ascertained at what point, or over what space, the abnormal sounds are heard loudest, and whether they be or be not propagated in the course of the large vessels. Occasionally listening over the back and in the course of the descending aorta may be useful.

4. When, during the above examination, we discover a new source of impulse and of sound in one of the large vessels, this must be especially

examined, the limits of such impulse and sound carefully ascertained,—whether they be or be not synchronous with those originating in the heart,—their direction, &c.

5. Under ordinary circumstances, the respiratory do not interfere with the detection of the cardiac sounds; but where the former are very loud and the latter indistinct, it is useful to direct the individual to hold his breath for a few moments. Sometimes, the impulse and sounds of the heart are heard better by directing the patient to lean forward; they may also, if necessary, be exaggerated and rendered more distinct by directing him to walk up and down quickly, or make some exertion for a short time.

Of the Sounds elicited by the Circulatory Organs in Health and Disease.—On placing your ear over the cardiac region in a healthy person, you will feel a beating, and hear two sounds, which have been likened to the tic-tac of a watch, but to which they bear no resemblance. They may be imitated, however, very nearly, as pointed out by Dr. Williams, by pronouncing in succession the syllables *lupp, dupp.* The first of these sounds which is dull, deep, and more prolonged than the second, coincides with the shock of the apex of the heart against the thorax, and immediately precedes the radial pulse; it has its maximum intensity over the apex of the heart,—below and somewhat to the outside of the nipple. The second sound, which is sharper, shorter, and more superficial, has its maximum intensity nearly on a level with the third rib, and a little above and to the right of the nipple—near the left edge of the sternum. These sounds, therefore, in addition to the terms first and second, have also been called inferior and superior, long and short, dull and sharp, systolic and diastolic,—all which expressions, so far as giving a name is concerned, are synonymous.

The two sounds are repeated in couples, which, if we commence with the first one, follow each other with their intervening pauses, thus—1st, There is the long dull sound coinciding with the shock of the heart; 2nd, There is a short pause; 3rd, The short sharp sound, and 4th, A longer pause,—all which correspond with one pulsation. In figures, the duration of these sounds and pauses by some have been represented thus,—the first sound occupies a third, the short pause a sixth, the second sound a sixth, and the long pause a third. Others have divided the whole period into four parts; of which the two first are occupied by the first sound, the third by the second sound, and the fourth by the pause. The duration, as well as the loudness, of the sounds, however, are very variable even in health, and are influenced by the force and rapidity of the heart's action, individual peculiarity, and form of the thorax. Their extent also differs greatly. They are generally distinctly heard at the precordial region, and diminish in proportion as we withdraw the ear from it. They are less audible anteriorly on the right side, and still less so posteriorly on the left side. On the right side posteriorly they cannot be heard. Their tone also varies in different persons; but in health they are free from a harsh or blowing character.

Great diversity of opinion has existed regarding the causes of these sounds,—all of which you will of course have heard discussed before coming here. You must never forget, however, the cardiac actions which coincide with them; for our reasoning from any changes we may detect, will entirely depend upon our knowledge of these. We may consider, then, that there coincide with the first sound,—1st, The impulse, or striking of the apex against the thoracic walls; 2nd, Contraction of the ventricles; 3rd, Rushing of the blood through the aortic orifices; and 4th, Flapping together of the auriculo-ventricular valves. There coincide with the second sound,—1st, Rushing of the blood through the auriculo-ventricular valves; and 2nd, Flapping together of the aortic valves. Contraction of the auricles immediately precedes that of the ventricles. The result of numerous pathological observations, and of many experiments, is that, in health, the first sound is produced by the combined action of the auriculo-ventricular valves of the ventricles, and of the rushing of the blood, which sound is augmented in intensity by the impulsion of the heart's apex against the thorax; whereas the second sound is caused only by the flapping together of the sigmoid valves.

With the cardiac as with the respiratory sounds, the alterations which take place during the disease may be divided into,—1st, Modifications of the sounds heard in health; 2nd, New or abnormal sounds.

I. *Modifications of the Healthy Sounds.*—These refer to the variations the healthy sounds present in their seat, intensity, extent, character, and rhythm.

Seat.—The sounds may be heard at their maximum intensity *lower* than at the points previously indicated, as in cases of dilated hypertrophy of the left ventricle, enlargement of the auricles, or of tumours at the base depressing the organ. They may be *higher*, owing to any kind of abdominal swelling pushing up the diaphragm. They may be more on *one side* or the other, in cases where the heart is pushed laterally by effusions of air or fluid in a pleural cavity. Various other circumstances may also modify their natural position, such as tumours in the anterior or posterior mediastinum, aneurisms of the large vessels, adhesions of the pericardium, deformity in the bones of the chest, &c. &c.

Intensity and Extent.—These are *diminished* in cases where the heart is atrophied or softened; when there is pericardial effusion, concentric hypertrophy of the left ventricle, or emphysema at the anterior border of the left lung. They are *increased* in cases of dilated hypertrophy, of nervous palpitations, and when neighbouring portions of the lung are indurated, especially in certain cases of pneumonia and phthisis pulmonalis.

Character.—The sounds become *clearer* or *duller* than usual according as the walls of the heart are thinner or thicker. Occasionally they sound *muffled* in cases of hypertrophy or softening of the muscular walls. Not unfrequently there is a certain degree of *roughness*, which is difficult to determine as being healthy or morbid. Occasionally it ushers in more decided changes; at others, continues for years without alteration.

Rhythm or Time.—I need not say that the frequency of the pulsations differs greatly in numerous diseases altogether independent of any special disease in the heart. In certain cardiac affections, however, the beats are *intermittent*, in others *irregular*—that is, they succeed each other at unexpected intervals. The *number* of the sounds also varies. Sometimes only one can be distinguished, it being so prolonged as to mask the the other. Occasionally three or even four sounds may be heard, depending either on reduplication in the action of the valves when diseased, or on want of synchronism between the two sides of the heart. Not unfrequently the increased and irregular movements of the organ, combined with the sounds, are of such a character as to receive the name of *tumultuous*.

II. *New or Abnormal Sounds:*—These are of two kinds: 1st, Friction murmurs; 2d, Blowing or vibrating murmurs. Dr. Latham has called them *exocardial* and *endocardial*. I am in the habit of denominating them *pericardial* and *valvular*.

Pericardial or Friction Murmurs.—The murmurs are the same in character, and originate from the same causes, as the friction noises connected with the pulmonary organs. It is only necessary to observe, that occasionally they are so soft as closely to resemble blowing murmurs, from which they are only to be distinguished by their superficial character and limited extent.

Valvular or Vibrating Murmurs.—These murmurs vary greatly in character,—some being so soft as to resemble the passage of the gentlest wind; others are like the blowing or puff from the nozzle of a bellows (*bellows murmurs*); whilst others are harsher, resembling the noise produced by *grating*, *filing*, *sawing*, &c. They are all occasioned, however, by diseases interfering with the functions of the valves. Sometimes these do not close, and the blood consequently regurgitates through them; at others, whilst this is the case, they are constricted, indurated, roughened, and even calcareous,—whence the harsher sounds. They may be single or double, and have their origin either in the auriculo-ventricular or arterial valves, or in both at once,—the detection of which constitutes the diagnosis of the special diseases of the organ. Occasionally these sounds resemble *musical notes*, more or less resembling the cooing of a dove, singing or twittering of certain small birds, whistling, tinkling, &c. &c. These depend either upon excessive narrowing of the orifices, or upon any cause which induce vibrations of solids in the current of blood, —as, when there are perforations in the valves, irregularities of their margins, string-like or other shaped exudations on their surface, &c. &c.

Auscultation of the large vessels.—On listening through the stethoscope placed over the arteries in the neighbourhood of the heart, we hear the same sounds as are produced at the sigmoid valves, propagated along its course, but more indistinct as we remove the instrument from the base of the heart. Those which are more distant have only one sound, which is synchronous with their impulse and their dilatation. This sound is of a dull character, but in health always soft.

In the various conditions of disease we have a single or double bellows sound, or it may be harsh, grating, rasping, &c. In the first place, you must ascertain whether any of these sounds are propagated along the artery from the heart, which you will know by listening over its course from that organ, and observing whether they increase as you proceed towards it. If the sound have an independent origin, it may originate from disease of the internal surface of the artery, when it will be harsh in proportion to the roughness; from stricture of, or pressure on the vessel, or from its dilatation. Generally speaking, the more dilated and superficially seated the vessel is, the sharper is the sound. Sometimes there is a double murmur in the course of a vessel, having an undoubted independent origin. This is most common in cases where there is an aneurismal pouch, into which the blood passes in and out through an opening narrower than the swelling itself. Occasionally one or both such murmurs may possess somewhat of a metallic ringing, or even musical character, when the margins of the opening are probably tense and thrown into peculiar vibrations.

I have already told you never to form a conclusion from auscultation alone. Even when combined with percussion, it is not safe to form a diagnosis without a knowledge of *all* the circumstances of the case. Hence, why I repudiate those rules which have been published in books, that have for their object the establishment of opinions from physical signs alone. At the same time, there can be no doubt that percussion and auscultation are absolutely essential to the proper investigation of maladies, although not more so than other modes of inquiry. I have, therefore, thought it best to give you a condensed resumé of the sounds which may be heard by auscultation of the lungs, heart, and large vessels, pointing out a few of the diseased states in which they may be sometimes (not always) heard, and especially indicating the physical conditions on which they are supposed to depend. Their true diagnostic value can only be learned by the careful examination of individual cases. —*Monthly Journal of Med. Science, Nov.,* 1850, *p.* 447.

30.—RHEUMATIC PERICARDITIS.

Reported by Dr. CHARLES KIDD.

[As cases of pericarditis are now so very frequently met with—being much more narrowly looked for than was the case formerly —and as the views of the nature and treatment of the disease have somewhat changed, the following may not be uninteresting to our readers.]

Few diseases are more obscure in their origin—few more insiduous and dangerous if neglected. The very essential nature of the organ engaged explains this, as well as the very general constitutional disturbance that always accompanies the affection. In rheumatic pericarditis, we have, possibly, two separate conditions of the system to contend against: one, a coagulable condition of the blood, in which the fibrin becomes increased in quantity, and is merely held in solution in the serum; the other, a state of actual arthritis, or acute inflammation of the joints, seldom or never going on to effusion or suppuration. An old opinion, coming again to the light, would ascribe the disease to a *materies morbi* in the blood, of which we know little, but that, as this fibrin increases in the blood in rheumatism, the blood corpuscles decrease, the fatty matters of the blood also increasing; that a development of lactic acid takes place at the same time in Prout's secondary assimilative process, always evinced in the deposits of the urine, in the perspiration, &c. Whether all these changes are sufficient to account for the great constitutional disturbance of rheumatism, or are merely links of a chain, beginning in a profound disturbance of the organic nervous system, and conveyed to the vascular system, and of course to the blood, may, perhaps, be still open to speculation. That a powerful impression by chills or cold on the nervous system, however, is the starting point of the disease in many cases, there can be no doubt. The disease is very common among the poor from this cause,—damp and cold, externally, preventing the generation of animal heat so connected with the proper functional activity of the organic system of nerves, influenced also and aggravated by atmospheric influences. The several complications we meet with depend, possibly, on pre-existing lesion, or disorders of the particular organs engaged; that of pericarditis, on some specific connexion between rheumatic inflammation of fibrous tissues, and the membrane surrounding the heart, which we cannot yet exactly explain.

The number of cases of pericarditis, in connexion with rheumatism, to be met in large hospitals like Guy's and St. Thomas's, must be familiar to the reader. Of course, the case following in this report, (selected out of many others), under the assiduous care of Dr. Barlow, cannot pretend to much novelty; yet, as exhibiting the more improved treatment at present used, and the general manageableness of such cases, it cannot fail to be of interest.

The treatment of pericarditis has recently undergone considerable modification. Taking blood by venesection, which would first suggest itself, though necessary in some cases, in others is all but inadmissible. It has been recently shown by Bouchardat, that rheumatism may even be induced by the coagulable state of the blood, the result of repeated bleedings. Every particular case, of course, will require a different line of action. Venesection after the middle of the first week, as a general rule, will lengthen the period of recovery, and many cases will not require bleeding at all.

The local abstraction of blood in rheumatic pericarditis will be always found that most beneficial. The treatment must vary, as we have to treat patients ill-fed and etiolated in cities, or healthy and full of red blood in the country. Antimony, alkalies, nitre, and colchicum, are very valuable. The exhibition of mercury has been lately dwelt upon, the general impression being that its beneficial effect will, in a great measure, depend on the period at which it is given. If early in the disease it will prove much more satisfactory than later, its action being more properly directed to prevent the deposition of organisable matter in the pericardium than to remove it afterwards. Indeed, our late statistics would go to prove that salivation has no very marked effect on the disease at all, the ceasing of the friction sound, according to Taylor, being more of a coincidence than really brought about through the agency of the mineral. As to the use of tartar emetic to remove inflammation, perhaps there is no difference of opinion; the dangerous effects of this valuable medicine having little or no existence except in the estimate of those who have not studied its effects.

[*Case.*—Margaret B., aged 20, single, had suffered formerly from rheumatism, and lately very much from debility, and irregular menstruation. She complained, on admission into Guy's Hospital, of burning hot pains in the joints, and all the accompanying symptoms of rheumatic fever. The heart's action was found to be quite normal, as well as the impulse, a very feeble bruit being perceptible with the first sound, particularly over the valves. The second sound free. Ordered pills with antim. tart. and calomel, and a draught for the morning, with senna and vin. colchici, to be kept very quiet, and to get simple drinks; without food. As the symptoms of pericarditis gradually came on, with acute pain below the left nipple, between the fifth and sixth ribs, a rubbing sound was detected with the systole, and occasionally of a "to and fro" character. Cupping glasses were now put on, and ℥vj. of blood drawn from the cardiac region, the former medicines being continued. Although the "rubbing sound" continued for six days longer, the general symptoms began to improve; but the bruit began to be traceable along the aorta. She was now, in addition to the same medicines, ordered a blister to the region of the heart. In a week after this, the pericarditic symptoms had entirely disappeared, although she had suffered some little relapse.]

It is not speculating, perhaps, too widely of our present purpose to say, if this young woman had been treated by any of the heroic plans of treatment formerly in vogue, she would be still a patient in a debilitated state in hospital. The practical fact for our consideration is, that bleeding lengthens out the period of recovery, and if not used early, so as to counteract the inflammation at its commencement, its use afterwards becomes more and more doubtful. The experience of different practitioners will, of course, differ with respect to this remedy and mercury, but a good deal may be explained by the character of the different patients under observation,—whether stout and well fed in the country, or, as is unhappily too often the case, ill fed and badly nourished in cities,—the safer rule, at least in cities, is not to debilitate further if it can possibly be avoided.

Pericarditis, under any mode of treatment, I need scarcely remark, is a very dangerous disease; the mere effusion may, by its pressure, so embarrass the heart as to bring on fatal consequences. Again, it may produce the most opposite results as regards this organ; atrophy, from pressure; hypertrophy and cartilaginous hardening of the walls of this viscus, and even fatal softening. When pericarditis does not end in resolution, the next best thing is possibly, adhesion of the opposite surfaces of the pericardium: this, also, is a very serious change in the organ. The constant association of endocarditis with inflammation of the pericardium explains, also, many of those organic diseases so fatal and so trying to the ingenuity of the practitioner. For all these reasons, indeed, pericarditis is a disease that cannot be too deeply studied, and that should not be allowed to go on one day longer than can possibly be helped. In renal diseases, and in rheumatism, we should always look out for it. Pericarditis is much commoner in children than generally suspected; no doubt laying the groundwork of those multitudinous cases of organic disease of the heart met with in after life. One of our pervading errors, perhaps, has been to look on rheumatism as a mere local disease, and pericarditis a metastasis. Nothing can be more erroneous.

Rheumatism is a strictly constitutional disease, and this very serious disease of the pericardium an integral portion of it, perhaps, from the beginning.—*Med. Times, Aug* 10, 1850, *p.* 143.

31.—CASE OF PURPURA, WITH REMARKS ON THE STYPTIC PROPERTIES OF OIL OF TURPENTINE.

By Dr. WILLIAM BUDD, Physician to the Bristol Infirmary.

[M. A. Riley, widow, aged 65, of spare habit and broken health, and very infirm. For the last twenty-one months she has suffered much from want, being often without sufficient food for days together, besides which, asthma and cough, attended with profuse expectoration, have added much to her weakness.]

Five days before admission she discovered, on getting out of bed, that her body and limbs had become thickly sprinkled with dark purple spots; she also observed that her urine was bloody, and in the course of the same day she began to bleed profusely from the mouth. Her stools were like soot. She had, in fact, become the subject of *purpura hemorrhagica* It would appear from her account, that these phenomena came on quite suddenly,—a fact not new to the history of the disease, but probably of some importance as to its pathology.

On the following day she grew rapidly worse. Blood flowed from the mouth without ceasing, and much was also voided in the stools and urine. On the 17th of May she reached the hospital very faint and exhausted, having come on foot from her own house, about two gunshots off. Within an hour after her admission I was summoned to her, as she appeared to be fast sinking. I found her in a state of alarming collapse. Her body and limbs were thickly covered with the characteristic ecchymoses, whose peculiar purple colour first gave the name to the disease. These ecchymoses were especially numerous and thickly set along the shins of both legs and the corresponding aspect of both forearms; on the thighs and upper arms they were more widely scattered, and on the trunk more widely still. In their general arrangement they were strictly symmetrical. As furthur illustrative of this, it may be mentioned, that there was an ecchymosis in the outer corner of each eye, and one at each angle of the mouth. The centre of those at the corners of the mouth was occupied by a small slough in process of separation. In the interior of the mouth, at a spot corresponding to the centre of the right cheek, there was a similar slough, nearly an inch long, and about half an inch broad. Blood was oozing very freely from under the detached border of all these sloughs. On the outside of the cheek was a large bruise mark, corresponding to the seat of the slough within. Numerous large blotches or bruise marks of the same kind were plentifully scattered over the surface generally. In addition to these, the following particulars were noted in the course of the day: gums firm and of natural colour; stools sooty and like coffee-grounds; urine the colour of blood, highly albuminous, and throwing down on being allowed to stand, a layer of grumous blood, occupying about one-sixth of the whole depth of the fluid.

Face pallid, without yellowness, and, with the exception of the large blotch already mentioned, free from ecchymoses.

Tongue and interior of mouth much blanched.

Debility extreme; faintness on every change of posture; tongue tremulous; pulse 100, small and weak; no febrile movement; appetite tolerably good; thirst natural; no enlargement of liver or spleen; extremities cold.

Cough frequent; expectoration copious; sputa opaque, greenish, diffluent; physical signs of a high degree of emphysema, with catarrhal râles.

I ordered her to be put on a generous and varied diet, including meat and vegetables, milk, and eight ounces of wine.

In the shape of medicine, she was to take two scruples of chlorate of potash daily, and ten grains of extract of krameria, in a wine-glassful of the decoction of logwood, every four hours. My colleague, Mr. Morgan, directed the bleeding sloughs to be "mopped" frequently with a dossil of lint soaked in oil of turpentine.

The effect of this measure was immediate. The bleeding ceased from the moment of the first application of the turpentine, and did not afterwards return; the sloughs soon came away, and in a few days, under the continuance of the same treatment, the sores were healed.

On the other hand, there was not the slightest check put to the other symptoms. The urine continued to be as much charged with blood as ever; the evacuations as black and sooty: and fresh ecchymoses made their appearance in great number from day to day. Wherever any, even the slightest, pressure was made, a large bruise mark was sure to be seen shortly after. The bodily weakness increased rapidly, and it soon became plain that, unless means could be taken to stay the loss of blood, the poor woman could not survive many days longer.

Matters being at this pass, on the 21st, after four days' trial, the chlorate of potash and astringents were given up as of no avail, and in lieu thereof twenty minims of oil of turpentine, in emulsion, were ordered to be taken every six hours. An immediate amendment followed the change of the plan. Before four doses of turpentine had been given all hemorrhage had ceased. On the following day the urine was pale and transparent, free from albumen, and every other trace of blood; blood ceased to appear in the evacuations from the bowels, and no new ecchymosis occurred on the surface of the body.

On the 28th, seven days after beginning the use of turpentine, the blood extravasated into the conjunctivæ had nearly disappeared, and the spots over the body generally had much faded.

Meanwhile, the patient had gained greatly both in flesh and strength. Her recovery suffered no interruption, and on the 21st of June she left the hospital much improved in health and condition. The turpentine was continued for a fortnight only. On the 4th of June, there being no trace of purpura remaining, it was given up, as of no further need. On the 25th May, the evening dose was vomited. One drop of creosote was, in consequence, added to each dose of the emulsion, and no sickness occurred afterwards. On the 28th there was some strangury, but this ceased at once on a slight reduction of the quantity of turpentine taken.

I shall not detain the reader from what he must already see to be the chief point of interest in the case just related, by any lengthened comments on the subject of purpura generally. Materials for the true pathology of this disease are as yet wanting, and it is not pretended that this case furnishes any addition to them. On this matter I will content myself, therefore, with a single remark. Purpura is no doubt correctly described as being, in the main, a disease of the blood. But, although the blood may be principally in fault, I think it is going too far to assume, as many do, that the altered state of this fluid is the *direct* and sole cause of its remarkable tendency to escape from the vessels. That it is in part the cause of this tendency, there is no reason to doubt; but that it is wholly so, is not proved. Although nothing of the kind has as yet been shown by direct observation, it seems to me more than probable that weakness of the vessels themselves, from defective nutrition of their walls, has a still larger share in the result. The symmetrical distribution of the ecchymoses is a fact which is, on the whole, strongly in favour of this view. Did time permit, I could mention others, which seem scarcely to admit of any other conclusion.

Equally foreign to our present purpose would it be to enter at length into the question as to whether there be any relationship—pathological or other between purpura and scurvy. To show that these two diseases are essentially distinct requires no laboured proof. It is enough to appeal to the striking contrast between them touching the circumstances under which they are known to arise. It has long been finally established that the one and sole cause of genuine scurvy is prolonged absence from fresh vegetables. Persons even moderately supplied with fresh vegetables, or their expressed juices. never get scurvy. Persons ever so abundantly supplied with them may get purpura in its worst forms. The three most rapidly fatal cases of purpura I have ever seen, occurred in persons who for months before had lived chiefly on potatoes and greens.

In scurvy, fresh vegetables and their juices are a speedy and infallible cure; in the treatment of purpura there is no proof that they have any efficacy beyond that which they possess as necessary constituents of a complete and wholesome diet.

These differences are fundamental, and it is clear that the two diseases to which they belong must be distinct in their very essence. Both may be characterised, more or less accurately, as diseases of the blood; and, in both, there is a peculiar tendency in this fluid to escape from its vessels; but here all similarity ends.

These considerations are, however, wide of our present mark. The interest of the case of M. A. Riley, is, I need scarcely say, wholly practical, and lies in its value as a striking illustration of the styptic properties of the oil of turpentine. It would, of course, be highly unphilosophical to found any conclusions as to the efficacy of a remedy from the observation of its effects in a single case. The result which here followed so closely upon the administration of turpentine has, however, been observed before in other cases without number. In purpura, especially, its virtues have long been known. The late Dr. Whitlock Nicholl believed it to be quite a specific for this disease in its idiopathic form, and to judge from the case before us, he was not so far wrong. I may add, that since this case occurred, I have met with three others, in which hemorrhage of various kinds and from various sources ceased at once, on the administration of turpentine, after other medicines had failed. The first of these was also a case of purpura; the second, a case of hemorrhage from the nose and kidneys: the third, one of hæmoptysis, from tuberculous disease of the lung. In the last, the outpouring of blood was truly appalling. The patient had already coughed up more than two quarts (by measure) before the first dose of turpentine was given; and yet, in spite of this tremendous loss, the bleeding was still going on without check or abatement.

It is right to add, that the power of this agent to staunch blood is no new discovery. The merit of it belongs, it would appear, to a provincial surgeon,—a Mr. James Yonge, an eminent practitioner, who lived at Plymouth in the time of Charles the Second. Mr. Yonge first made his observations public in the year 1679, in a pamphlet, bearing the quaint title of 'Currus Triumphalis è Terebintho,' or an 'Account of the many admirable Virtues of the Oil of Turpentine.'

[Besides the testimony of many other observers, the illustrious John Hunter, speaking of styptics generally, says,]

"A dossil of lint, dipped in oil of turpentine, after having first wiped the wound clean, in order that it may reach the vessel, is the best, and may be renewed pretty often. I have seen it immediately stop vomiting of blood from the stomach, after all other means had failed, given internally with white of egg, as often as the stomach would bear it. In external hemorrhages, where it had not the desired effect applied externally, I would give it internally. *It is the best, if not the only true styptic.* Thus in a case of nasal hemorrhage, which nothing would stop, I gave ten drops of oil of turpentine in a draught, and repeated it every two or three hours, which entirely stopped the bleeding in less than twenty-four hours, and it never returned."

The testimony of Mr. Vincent, late senior surgeon to St. Bartholomew's is to the same effect, and equally strong. After making some observations on the subject of the hemorrhagic diathesis, this experienced surgeon goes on to say—

"A most desirable object would be obtained by securing some remedy that would alter this peculiar disposition in vessels to bleed, and arrest the loss of blood; so that it may not continue to endanger the life of the patient. I have so invariably found turpentine infallible in effecting this intention, that it may, I think, be depended upon as a most valuable remedy.

"Some years ago a youth was brought to me who was passing blood in his urine. I ordered some draughts, with a few drops of oil of turpentine; the bleeding quite stopped before the end of the second day, and did not return. About a twelvemonth afterwards he was brought to me, having cut his finger but slightly: it had continued bleeding for some days. I gave him turpentine again; it stopped in a day or two. Not long after he came a third time to me; he had a tooth extracted, and it had been bleeding for several days. The turpentine was had recourse to, and the remedy soon acted in the same sanitary way. I have several times been called in on account of hemorrhages where teeth have been extracted, and have never seen the turpentine fail in this nor in other similar cases of hemorrhages."

The reader will not fail to remark, that the evidence which has now been laid before him, not only tends to show that oil of turpentine is possessed of styptic powers of a high order, but that, if this evidence be good, these powers are effectual in issues of the blood from sources the most various, and arising from causes the most diverse. Tuberculous hemoptysis, nose-bleeding, hematemesis, purpura, and traumatic hemorrhage, have little else in common, beyond the fact of an unnatural escape of blood from its vessels. The effects of turpentine in purpura, therefore, are most probably directed, not against purpura as purpura, but against purpura as hemorrhage. Bearing this in mind, I beg it to be also borne in mind, that in the case of M. A. Riley, not only did the bleeding from the kidney cease immediately after its administration, but that from the intestine also, as well as the tendency to extravasation from the vessels of the skin.

Another very important fact, that would also seem to be established by this evidence, is, that turpentine is even more effectual in arresting hemorrhage, when directly applied to the bleeding surface than when it is given internally. From these facts taken together, the inference would seem to be pretty secure, that its virtues as a styptic must depend on its exerting some direct influence on the contractile power of the blood-vessels. In the elementary nature of such a property we seem to find the best, and indeed the only, plausible explanation of the equal efficacy of this medicine in restraining bleeding under such a great variety of pathological conditions. In the adoption of this view, we also obtain, if I mistake not, the best guide to the cases in which its administration is most likely to be successful They are, on the one hand, the great group of hemorrhages, which are generally characterized by pathologists by the epithet *passive;* and, on the other, those in which, in addition to its general action, the remedy comes into direct contact with the surface from which the blood is issuing. I have had no experience of its effects in hematemesis arising from simple ulcer of the *stomach*, but from what we witnessed in the case of our patient's *mouth*, I can readily believe that Dr. Seymour is right in the conclusion to which he has come, that in that disease it not only tends to stop the bleeding, but also promotes the healing of the sore.

In what are called active hemorrhages, it would not seem to be so suitable a medicine. We must not, however, be hampered by definitions like these in cases where life is threatened. Cases of hemorrhage are cases in which, beyond all others, the nice distinctions of pathology have to give way before the great emergencies of practice.

In the treatment of uterine hemorrhage, whether occurring in connexion with parturition or not, the employment of turpentine, both topical and internal, admits, I have no doubt, of much more extensive application than it receives at present. Of its great use in severe menorrhagia from common causes I can speak from my own experience; and in the 'Provincial Medical and Surgical Journal' for the present week, Mr. Griffiths, of Wrexham, gives a series of cases, from which it would appear that, in the dose of half an ounce or an ounce, repeated once or twice, according to circumstances, this medicine often succeeds in arresting at once the flooding which is apt to occur after delivery, and which is sometimes so appalling.

In common cases, such as those of purpura, for instance, where time does not press, I have found doses varying from ten minims to half a drachm quite effectual; but in cases of rapid loss of blood, where, as sometimes happens, every minute is of importance to life, it is better to give half an ounce or an ounce at once, to be followed up by smaller doses at shorter intervals.

I should mislead you, were I not to add, before concluding, that the use of this medicine is not without its drawbacks. Few great gifts come to us without alloy, and turpentine is no exception to the rule. In the first place, there are few drugs more repugnant to delicate stomachs. In many, it excites vomiting; in others, it causes painful strangury; and the irritation of the urinary organs is sometimes so great, that blood is passed with the urine.

Some of these evils may, however, be avoided, or much diminished, by adopting a particular mode of administering the medicine. Thus, I have found that the addition of half a drop or a drop of creosote to each dose almost entirely covers its nauseous taste, and prevents the risk of sickness. Creosote itself is not, it must be owned, the most palatable of drugs, but neither is it nauseating. Ten or twenty drops of spirit of rosemary will help to make the prescription less disagreeable; and of the different vehicles proposed for keeping the turpentine in suspension, I think you will find common mucilage the best.—*Medical Times, August* 17, 1850, *p.* 167.

32.—*Conclusions respecting the Seat and Nature of Angina Pectoris.* By Dr. KNEELAND, U.S.—Dr. Kneeland draws the following conclusions from his researches into the nature and causes of angina pectoris:

1. From the symptoms and morbid appearances, angina pectoris is not a disease of the lungs, heart and its vessels, or stomach, but an affection of the nerves supplying these organs.

2. Anatomy, physiology, and pathology would lead us to place the seat of angina pectoris in the par vagum, and not in the sympathetic system of nerves.

3. Like other nerves, the par vagum may be affected with neuralgia and rheumatism, with inflammation; it may be compressed by morbid growths; its spinal origin may be compromised by hemorrhage, accidental wounds, and various irritations—all of which may cause the symptoms of angina pectoris.

4. Angina pectoris and asthma are intimately related: the *former* being an affection more especially of the *sensitive* filaments of the par

vagum; and the *latter* an affection of its *motor* filaments. Both are generally more or less combined in the same case.

5. Angina pectoris is a disease not necessarily fatal, especially in young persons, if accurately diagnosticated, and properly treated.

6. In addition to the remedies of the books, special attention should be given to the inhalation of oxygen, and to the use of electricity.

7. In cases of angina pectoris, attention should be directed to the examination of the par vagum, from its origin to its terminations, which, doubtless, on careful examination, will exhibit lesions sufficient to account for a fatal result.—*Amer. Journal of Med. Sciences, Jan.* 1850, *p.* 271; *Med. Gazette, Aug.* 16, 1850, *p.* 271.

DISEASES OF THE ORGANS OF RESPIRATION.

33.—ON TUBERCLE.

By John Simon, Esq., F.R.S., &c.

[Mr. Simon describes tubercle as an organizable deposit occurring in various organs under a peculiar constitutional state, termed the scrofulous diathesis. He says:]

Under the influence of a constitutional bias, which is peculiarly susceptible of hereditary transmission, and peculiarly difficult of artificial generation, there occur local deposits of an organic material, which is insusceptible of ulterior development.

[Mr. Simon considers that the debility which frequently accompanies manifestations of the scrofulous diathesis is not essential, but accidental, merely from the fact that the scrofulous patient is generally under the influence of debilitating causes; but that the exposure to these causes, if no hereditary tendency exists, will not produce tubercle, until at least several generations have been thus affected; and Mr. S. cautions us as to the too implicit belief in "the experimental production of tubercle." He says with regard to the production of tubercle in the livers of rabbits, by confinement in a damp dark cellar, with watery vegetable food, that what he has been shewn confidently as tubercular deposit, he has found, by employing a high power of the microscope, that the mass consisted of the minute oblong eggs of some entozoon. Not only has this been the result in a few, but in dozens of cases, and the same ova have been observed in the gall bladder, and in the whole length of the intestinal canal, lining some of the follicles like an epithelium. So far as his experiments have proved, he has never seen tubercle in the rabbit. Mr. Simon then proceeds to the consideration of hereditary transmission. He says:]

Here, however, let me detain you on the subject of the *hereditary transmission* of scrofula, and explain to you what I mean, when I speak of its being continued in this manner from generation to generation. I do not mean that, in the process of procreation, actual tubercular matter passes from the system of the scrofulous father into the germ of the infant, to remain latent there, till circumstances call for its development; nor, that during uterine life, the blood of the child is poisoned by its mother's blood, as occurs in small-pox or syphilis. What I mean is this:—that the scrofulous diathesis—that the *disposition to form tubercles* is transmitted; that the child inherits an imperfect pattern of development.

I must explain this more fully, for the inheritance of *dispositions to disease* does not belong to scrofula only; it forms a very important problem in the pathogeny of cancer with its allied disorders (for they are often hereditary) of gout and rheumatism (for they, too, are heirlooms in families); and it accordingly constitutes one of the most considerable questions in the study of General Pathology.

Every one recognises in the process of generation a certain amount of hereditariness—a certain amount of that influence by which a parent becomes the pattern of formation for his child. One allows for accidental deviations—for hare-lips, for club-feet, even for an occasional acephalism; but, these allowances being made, one prognosticates with great certainty as to the result of any particular sexual intercourse. No man expects to become the father of an armadillo, of a flying-fish, or a stag-beetle; nor (except in the Arabian Nights) do royal husbands believe that their princesses can be accouched of logs of wood. But more than this, it is expected, and, on the whole, very generally realized, that the child shall be more like its father than its god-father. So far the case is clear; but I wish you to observe the tendency further. Follow the child in its ulterior development (for that is the point) and mark how exactly, in various exterior and noticeable signs, he repeats the development of his father; how, in arriving at the age when his father got corpulent, he acquires the same figure; how, at the age when his father became grey, or bald, he, too, becomes grey or bald, and with the same succession of parts—vertex first, or temples first, or forehead first, as the case may be; how his teeth decay, or drop, or protrude, just as his father's; how his pulse is of the same general character—even, as I have often noticed, to the degree of copying an intermittent rhythm (not accounted for by organic heart-disease); and how his habits of sleeping and waking follow the same direction.

Now, observe, (for the distinction is of great importance) that these things are not *connate;* the child is not born a copy of his father as he begat him; but he is born, having his father's past development as a type for his own future development, so that he shall be developed as his father was developed, and shall hereafter become like him. In addition to that *general law* of human development, by virtue of which he is destined to be a mammal rather than a bird; and a man rather than an ox; and to reach puberty, manhood, old age, and death in a certain defined succession; he is further possessed by an inherited *personal and particular law* of development, which affixes a something peculiar and individual to his passage through each period of his existence.

If my meaning in all this has been intelligible to you, you will readily conceive that diseases *affecting the development of the body* are peculiarly those which would transmit themselves in the line of hereditary succession; that the disposition to a disease would be hereditary, where the disease consisted in the results of a vicious type of development, affecting limb or viscus, solid or fluid in the body; and that no disease would be hereditary except in so far as it might be developmental.

Practically, this is notorious. There used to be a comical print in the shop-windows, with the inscription "a chip of the old block;" it represented an old seaman having his infant son presented to him; and this new-born individual was made to carry the family likeness by having a wooden leg exactly like his father's. Every one could smile at this absurdity, because of the general recognition of what I have just stated to you. Accidental mutilations do not become hereditary; for many centuries the Jews and the Mohammedans have undergone circumcision of the prepuce; but the local deformity has never transmitted itself; the new-born Jew or Mussulman offers probably as much foreskin to the shears, as the immediate successors of Abraham or Mohammed. If, on the contrary, one could so regulate the embryonic development of a human being, that it should be born having one leg, or no prepuce; and still more, if one could repeat this modification of development for two or three generations; then, I entertain no doubt that the peculiarity acquired in these generations would transmit itself indefinitely in the future productions of the race.

And, if you transfer these arguments to the several varieties of disease, you will know, on the one hand, in what diseases to anticipate hereditary transmission; and, on the other hand, what peculiar character of disease (to wit, its developmental character) may be inferred from the fact of its hereditary succession. Of a disease like scorbutus, or ague, dependent on the presence or absence of certain exterior accidental conditions, you would anticipate that it could not be hereditary, any more than a wound or a dislocation. Of those accidental accompaniments of scrofula—the morbid processes which arise in defective nutrition (the ulcerations of the cornea or intestines for instance, which depend on insufficient or inappropriate nourishment) and which, as I have said, are essentially co-extensive in their duration with the exterior causes which produce them—you would know that they could have no natural tendency to perpetuate themselves in this way. Of another disease, on the contrary—one like plethora, relating essentially to the rate or degree of development in an element of the body, you might predict that it would tend to become hereditary. And whereas it is in the blood, more strikingly and more constantly than in any ingredient of the organism, that development is ever in progress; whereas, it is eminently in the blood, that we have at each moment an epitome of the whole development of the body, and find the earliest rudiments and the latest reliques of every organised tissue, nascent or in decay; so surely it would be in this fluid —the scene or the subject of so many developmental metamorphoses, that one would expect to find the material explanation of many hereditary diseases. One would expect that an inherited disposition to form, at various periods of life, and in a number of different organs, certain special and characteristic materials, bearing definite relation to the normal products of the body, would indicate a peculiarity in the development of the blood, whereof those deposited materials would be the result and the expression.

Such is the state of the case in regard of scrofula; and therefore it is, that I have analysed this question of hereditary tendencies to disease. Strict experiment would not, I think, justify me in telling you *as a certainty,* that the scrofulous diathesis has its explanation in such ground as we have gone over; but, though we are short of absolute demonstration on the subject, I may tell you this with confidence: there exist many facts rendering it highly probable that tubercle has its rise in disease of the blood; that this disease of the blood is one affecting its development; and that it is as a *developmental disease of the blood* that scrofula acquires its tendency to hereditary succession—its tendency to perpetuation as part of a family likeness. I shall presently give you other evidence in support of the same view.

Meanwhile, to return for a moment to the narrower ground from which we started, remember, that what is meant in calling scrofula an hereditary disease, is—not that the tubercular material is to pass from parent to child—not that the child is to be born with tubercle already in its body, but that the disposition to *form blood in a manner which shall give tubercle as a collateral phenomenon,* exists as a clause in the child's charter of life, and forms a part of its type of development, as truly as any exterior resemblance which he may bear to the configurative growth of his parents.

[Amongst the female phthisical patients at the Consumptive Hospital last year, 36 per cent. reported their parents as having been consumptive. The importance of this statement will be apparent, especially when we remember that in the remaining 64 per cent. there may have been a considerable proportion whose parents had not, indeed, suffered from tubercular phthisis, but may have suffered from tubercular deposit predominantly in other organs than the lung; and that there may have been a second considerable proportion whose parents may have escaped tubercular disease in their own persons, but may yet have transmitted the predisposition from their own immediate predecessors to those later inheritors of the disease.]

With respect to the material of tubercle, you so often have opportunities of seeing it, that I need hardly detain you with any elaborate description of its characters. "When in that condition that its properties are most clearly marked, and when at that period of its development that no dissentient opinions are held as to its nature, it presents (says Dr. Walshe) the following characters:—It is an opaque substance, of yellowish colour; sufficiently firm, yet friable; of little tenacity, and resembling cheese very nearly in point of consistence; inelastic; without particular smell; accumulated in small masses, varying in size from a pin's head to a hen's egg; of homogeneous aspect all over their divided surface; exhibiting no vessels; insoluble in water, and, if mixed therewith, quickly subsiding to the bottom. And these are the properties of a material which, in respect of its physiology, is characterized by its tendency to become soft, after it has existed for a variable period in the condition of firmness, and to induce various changes in the natural textures with which it is connected—changes eventually affecting its own complete disintegration and elimination. In the same natural texture with such tuberculous matter as we have now described are very frequently found certain small bodies, varying in size from that of a pin's head to a very small pea, of greyish-white or greyish tint, and glistening aspect. These bodies occur in different organs and textures, in association with yellow tubercle; they are more or less transparent; and though in their own substance of light-greyish colour, their translucency sometimes gives them, in appearance, the tint of the circumjacent structure; their section exhibits a smooth and close surface; hard as cartilage, almost, in some instances, and invariably remarkable for firmness; in general outline seeming roundish, yet in reality of somewhat angular form, and adhering so closely to the adjoining tissues, that they cannot be removed without particles of these; they have a striking tendency to accumulate in groups."

Such, then, are the two conditions in which the scrofulous product may show itself,—either as the *yellow tubercle*, or as the *semi-transparent grey granulation;* and as regards the relations of these to each other, it appears that (in organs where both occur) the grey granulation is the earlier deposit; that it may be seen alone; that the yellow material may be seen in its substance, and may gradually take its place; that, in short, it may fairly be considered to constitute a first stage of the local disease, and to be preparatory for the more characteristic yellow deposit. This, however, must not be understood, by any means, as of universal applic tion; for there are many organs (as, for instance, the lymph-glands and brain) in which the grey granulation has rarely or never been seen, although the yellow tubercle is among their frequent diseases; so that the former deposit cannot be considered necessary for the development of the latter. It is especially in the lungs, that the grey granulation is habitually encountered; and in these organs unquestionably it seems to form a point of attraction—a matrix or a nucleus—for the yellow deposit.

The material of tubercle habitually presents itself in the shape from which its name is derived—namely, as *tubera* or rounded bodies, varying from the size of the smallest pin's head to that of an orange; evidently attaining their larger development by means of exterior accretion; and tending, where numbers exist together, to coalesce into irregular (though still tuberiform) masses. Where the individual tubercles have reached the size of the smallest pea, their substance consists almost invariably of the yellow material. In some organs, the structure which intervenes between separate tubercular masses, will show to the naked eye no other than its normal characters; but in the lung it is frequent to find considerable inflammatory condensation of this intervening tissue. Sometimes, too, in this organ a large irregular extent of structure will have lost its natural sponginess, will have become dense and impervious to air, though still moist and compressible, and will present, on section, a nearly homogeneous, glistening, semi-transparent section, at various points of which one may perceive the deposit of yellow tubercular material. It is this latter condition—one by no means frequent in its occurrence—which is known by the name of tubercular infiltration; it apparently consists in the accidental presence of true tubercle in the midst of common chronic hepatization of the lung from pneumonia, and may be supposed to depend on a partial tubercular change of some prior inflammatory exudation.

Tubercular deposit in the lymphatic glands is in so far an exception to the general description I have given, as not to present itself at first in the spherical or tuberose form. Sections of a scrofulous lymph-gland, made when its mass is only partially tubercular, show an irregular yellow blotting of the cut surface, not arranged in circles or segments of circles.

Masses of yellow material, like firm curd-cheese—existing either in a concreteness which allows it to be squeezed from the diseased organ, as though from a follicle, or else diffused in the substance of such an organ, and blended with its structure, so as not to be isolable by pressure or dissection; and smaller masses of semi-transparent grey material, extremely firm, and presenting a close resemblance to certain forms of fibrinous concretion observed on the valves of the heart: these, then, are the two sorts of deposit habitually noticed in the tissues of scrofulous persons; and it will be obvious to you that the first-mentioned is the characteristic and distinctive product.

Microscopical examination of tubercle shows the following principal ingredients: (1) a substance which constitute distinctively the bulk of the grey granulations, and which in its general character is identical with the matter of condensed fibrinous concretion; namely, a dense, transparent, and almost homogeneous stroma, soluble in acetic acid and in the alkalies: (2) granular material, often in overwhelming abundance, especially in the yellow tubercle (where it is superadded to the former constituent) and consisting partly of fibriniform granules, partly of molecular oil: (3) aborted cyto-blasts, dark, condensed, mis-shapen, angular, insoluble in acetic acid.

These, it appears to me, are the characteristic elements of tubercle; and, in addition to them, we habitually observe the admixture of other and accidental products—viz., (4) calcareous granules, frequent in some forms of tubercle, representing the material by which they are liable to cretaceous transformation, and consisting mainly of the phosphate and carbonate of lime; (5) shapes evincing the various stages of pus-development, and derived from inflammatory changes in the adjoining non-tuberculized parenchyma of the organ—changes due to the proximity of the tubercular concretion, and manifested chiefly at the period of its softening; (6) especially in the lung, a variety of cell-forms native to the organ; epithelium of larger and smaller bronchial tubes, ciliated, and non-ciliated, often presenting partial abnormalities of growth due to chronic irritation of the mucous membrane; (7) blackening carbonaceous deposit, which is especially common about such grey granulations as are becoming obsolete; (8) plates of cholesterine and glomeruli of oil, mixed in the detritus of softened tubercle; (9) remnants of the original tissues through which the tubercular deposit has been infiltrated; as, for instance, in the lung, fragments of elastic fibre.

No formation of new blood-vessels ever occurs in connexion with the processes of tubercular deposition. On the contrary, each mass as it forms, effects a complete obliteration of all the capillaries within its sphere of infiltration, so as to become utterly non-vascular. In the concrescence of several tubercular masses, intermediate arteries and veins may remain pervious, and continue to maintain the circulation of blood in parts more distant; but the tubercle itself, however large, has no trace of capillary circulation in its interior; it is essentially bloodless.

You will observe, in this enumeration of the elements of tubercle, that I make mention of no characteristic cells. I know of none such. I believe, that so far as a given mass is tubercle, so far it is incapable of originating or maintaining any process of cell-growth; and I suspect that some observers have fallen into the error of describing, as characteristic of tubercle, cells which have been derived from the inflammation of adjoining textures. With exception of those large glomeruli or granuliferous cells, which are found in the fluid of softened tubercle just as in deliquescent venous coagula) appurtenant to the disintegrating process, but neither characteristic of tubercle nor habitual to it at other periods of its existence—with exception of these (which are probably of extrinsic origin) and of the undoubted products of inflammation furnished by the vicinity, I am ignorant of any cell-growth associated with tubercle.

The aborted and stationary cyto-blasts which I have mentioned, are very general in tubercle, and probably represent modified natural elements of the lymph and blood. I should hesitate to speak of them as invariably and diagnostically present, but I do not know that I have ever failed to see them. They are not particularly abundant in the grey granulation.

The ulterior changes of tubercular matter, when once it has been deposited in mass, are almost invariably in the direction of deliquescence. Exceptionally, the grey granulation may become horny, hard as a shot, and incapable of further intrinsic change; or the yellow mass may lose its animal ingredients, and become the seat of a permanent calcification; but, speaking generally, tubercle tends to soften by some chemical conversion of its material, and thus, in a liquified form, to be discharged from the system by the ulceration of surrounding parts. It is this process, long confounded with suppuration, with which you are familiar in the process of phthisis, as leading to the formation of vomicæ, and as continuing, in an infinite majority of cases, to form cavity after cavity in the lung, till the patient sinks, exhausted by their discharge and irritation. It is the same process, again, which leads to the formation of scrofulous abscesses in the lymph-glands of the neck or mesentery, and in other organs of the body; where we have opportunities of observing the curative processes (identical with the healing of ordinary abscesses) which ensue in parts not of vital importance, when the tubercular material has been enabled to discharge itself.

In the progress of softening, the stroma of the tubercle vanishes; the molecular oil and other molecular matters increase; the involved elements of natural tissue break up, and add the products of their disintegration; serum and pus are contributed by the surrounding structures; glomeruli arise; and the whole mass, becoming saturated with fluid, shows, on microscopical examination, a mixture of organic detritus with incidental inflammatory products.

When this change has commenced, the circumjacent parts probably contribute to its completion, by furnishing an additional fluid which favours the decomposition of the tubercle; but the change is originated, as an intrinsic and characteristic one, in the tubercular material itself; commonly commencing in the interior of its concretion, where the product is of oldest formation, and where the influence of neighbouring tissues would of necessity be least. Its resemblance to the well-known softening of fibrinous coagula in veins is too remarkable to be overlooked —a resemblance both in the steps of the process and in its material results. This is rendered the more interesting from the obvious similarity which exists between tubercular matter and fibrin, in respect of most chemical characters and of some general physical qualities. There seems no reason to doubt that these processes are essentially identical, and that the pathological gist of scrofula consists in the deposition and ulterior deliquescence of a fibriniform material.

The often-asked question—Is tubercle an inflammatory product? seems to be answered in anticipation by the description I have already given you. In itself, it is clearly no product of inflammation, but is apt, especially at the period of its softening, to act as an irritant of surrounding textures, and to invite the addition of their inflammatory exudations. The evidence of its non-inflammatory origin—the evidence of its origin as a concretion—seems sufficiently furnished by the results of microscopical examination; and to this may be added the testimony of all clinical observers, that tubercular accumulations may advance latently, and may reach considerable magnitude or extensive diffusion, without producing in the patient any signs, local or general, of inflammatory action. It is important, however, to know that its mechanical interference with the circulation of blood is great. In the lungs—for some reason hitherto unexplained this obstruction to the passage of blood acts peculiarly, and leads to the occurrence of hemoptysis. We do not find hemorrhage habitually associated with tubercle in other organs of the body; nor do we find that other than tubercular deposits in the lung readily induce hemorrhage there; but this particular deposit in that particular organ produces the result with extreme frequency. In the first report of the Consumption Hospital it is stated that about two-thirds of their phthisical patients suffered from hemoptysis; and in nearly three-fourths of this number the symptom in question arose *before* the occurrence of softening; when, namely, it must have depended on the rupture of blood-vessels from their mechanical obstruction, and not on their being opened by any ulcerative process.

I have now to state to you the *order of preference* in which different organs are invaded by tubercular disease; and I may take this opportunity of saying explicitly (if I have not already done so) that I look upon the scrofulous deposit, which we so often find in the glands of the neck and in other lymphatic glands, as identical with the material of pulmonary and other visceral tubercles. Both chemical and anatomical examinations establish this view (in my opinion) so conclusively, that I am quite unable to understand how it is that some pathologists still hesitate to admit the fact. The description which I have given of yellow tubercle as it occurs in the lung, would apply with equal accuracy to scrofulous deposit in any other organ of the body.

As respects the choice of organs for scrofulous deposit, I can give you infinitely better authority than my own, in quoting the observations of Professor Rokitansky; his pathological statistics—founded as they are, on what he well calls "collossal materials," can be equalled from no other source. The annual necropsies in the General Hospital at Vienna probably exceed at least twenty-fold those which occur yearly in any one of our large London hospitals; and Rokitansky's great name has been connected with this unique school of pathology, I believe, during twenty years. In summing up figures for such statistical deductions as I am about to give you, his materials must consist of more than *tens of thousands* of instances. The following, then, on his authority, I give you as the order of frequency with which different organs are found tubercular in the dead body:—Lung; intestinal canal; lymph glands, especially the abdominal and bronchial; larynx; serous membranes, especially peritoneum and pleura; pia mater; brain; spleen; kidneys; liver; bone and periosteum; uterus and Fallopian tubes; testicle and its appendages; spinal marrow; voluntary muscles: and, in children, this order of precedence so far undergoes a modification, that the lymph-glands and spleen stand first in the list.

But, in order to be rendered a true transcript of nature, this list requires a very important qualification. It confounds together the primary tubercular deposits with those which have occurred as secondary formations, during the fatal progress of the disease; counting (for instance) tubercular ulceration of the intestines as an unit for the statistical table, whensoever it is found in the dead body, although perhaps it may never have been discovered, except as a posterior complication of other tubercular disease. The important question obviously is this,—by what organ does scrofula first possess itself of the system? not—what organs are oftenest found tubercular in post-mortem examinations, where several organs are simultaneously seen to be diseased? but—what organs are selected by nature for the first invasion of the disease? what organs give the greatest facilities for its characteristic deposit? Hear Rokitansky again:—The lungs and the lymph glands still maintain, in a very high degree, their numerical superiority to all other organs affected singly; while the intestines, larynx, and trachea, serous membranes, spleen, and liver, fall at once from the high level at which they stood in the less discriminative list; they fall so low, as to indicate, conclusively, that they are almost, or quite, insusceptible of the primary invasion of tubercular deposit;—that they suffer only in a secondary way, and are visited (so to speak) only by the superfluities of the disease.

This is matter of immense importance. The organs, in which the disposition to primary deposit almost exclusively prevails, are such as may, with equal exclusiveness, be called the *organs of blood development;* and in this generalization we possess a clue, which it is impossible to over-estimate, for arriving at a true interpretation of this fatal disease.

We have next to inquire, respecting the several organs in which tubercle occurs—what anatomical relation is borne by the morbid deposit to the natural tissues of the organ.

In the lymph-glands, I suspect that tubercle is in truth no *deposit;* that it is not derived from the blood in the vessels there ramifying, but is an accumulation in the tubes of the gland of their own morbidly coagulable or inspissated lymph; that it is therefore not, strictly speaking, a disease of the lymph-gland, but of the lymph in the glands.

As respects other organs, it seems that the readiness with which the tubercular material coagulates, prevents its passing to any distance from the vessels which have furnished it. Accordingly, we do not find it occurring in tissues which derive their nourishment by imbibition through some length of extra-vascular structure, as cartilage. But in organs which consist of involuted mucous membrane, with a more or less solid plexus of blood-vessels, we find that the deposit readily concretes on the free surface of the membrane; thus, in the lungs, it may be seen to lie as a secretion in the air-cells, till it has collected in sufficient quantities to obliterate their septa, and microscopically to appear as what may be termed tubercular infiltration. Among the observations which illustrate the first deposit of tubercle in the lung, I have much pleasure in citing those of my colleague, Mr. Rainey; and I recommend you to read attentively the paper on this subject which he communicated to the Medico-Chirurgical Society in 1845. In other compound mucous organs I have seen the same thing; for instance, in the follicles of the uterus, and in those of the vermiform appendage; while a mash of softened tubercle has been lying over the general surface of the mucous membrane, I have been able to isolate the subjacent follicles, to see them full of apparently tubercular matter, and yet with their definite limitary membrane quite uninjured. However, while I entertain not the slightest doubt that tubercular matter concretes readily within the air-cells, on the free surface of the limitary membrane (for on many occasions I have seen it occupying this position) I should hesitate to admit that this is its exclusive seat, even in the earliest commencement of the disease. In studying injected specimens of pulmonary tubercle, one sees many nebulæ of deposit, which show no trace of subdivision according to the arrangement of air-cells, and which yet do not appear of sufficient density to have caused the destruction of membranous septa. I am not able to tell you with certainty, whether the tubercular element of the blood may coagulate within the capillary vessels of the lung, and in their immediate proximity; but there are pathological reasons for considering this as not improbable, and as helping perhaps to explain the extreme power of obstruction which tubercle exercises on those vessels, and the singular tendency which it possesses, beyond all morbid interferences with the pulmonary circulation, to produce the symptom of hemoptysis.

Likewise, among anatomical peculiarities of this deposit may be mentioned, that in almost every organ there is a favourite spot, where the masses of tubercle first appear. Thus, in the lungs, there is the well-known preference for the summit; in the pia mater, for the region of the base, about the commencement of the optic nerve and the fissure of Sylvius; in the brain itself, for the grey substance; in the osseous system, for the cancellated structure; in the bowels, for the lowest part of the ileum; in the testicle, for the epididymis; in the female sexual system, for the Fallopian tubes and fundus of the uterus.

Next we have to inquire—What are the pathological affinities of tubercle? what disease does it refuse to co-exist with? with what diseases is it often associated?

First, for the negatives: Rokitansky, whose immense materials are again a sure resource, says: Tubercle hardly ever occurs in those who are the subjects of cystic tumours, such as those of the ovary: Bronchocele seems incompatible with it, and ague is alleged to be so: It is with the utmost rarity that tubercular patients are attacked with typhus: Tubercle and cancer are incompatible.

Lastly, I told you in a former lecture, that Rokitansky attaches great importance to an increased *venosity* of the blood, as an antagonistic condition to the formation of tubercle; and under this head he includes every influence which interferes directly or indirectly with oxygenization of the blood, either by diminishing the capacity of the chest, or hindering the expansion of the lungs, or by deranging the pulmonary circulation of blood, or by impeding the free access of air thereunto.

For example: a case of spinal deformity narrowing the chest; a case of abdominal tumour encroaching upward, and causing dyspnœa; a case of cyanosis maintaining deficient aeration of the blood; these would be cases in which, according to this observer of hundreds of thousands, the tubercular deposit would not arise.

With respect to those contrary classes of disease, with which tubercle most readily associates itself, there stand first of all, those atrophic, ulcerative, and sub-inflammatory processes, to which I have already adverted as apt to occur in depressed states of the system, and which maintain (as I explained at the commencement of my lecture) only an accidental affinity to tubercle. But there is one very important class of structural changes, which the tubercular constitution so habitually associates with itself, that I can hardly refrain from considering their mutual relations important or essential. I allude to what is called the fatty degeneration of the liver, the kidney, and the arteries; and I am disposed to believe that, when our knowledge of the subject has become more complete than it now is, we shall find proof that these fatty degenerations stand in some essential relation to the tubercle-forming diathesis—stand perhaps in the relation of secondary dependence on it. You will rarely make the post-mortem examination of a tubercular patient without finding an augmented formation of fat in one, two, or all of the three situations I have mentioned; in the liver, constituting its well known fatty enlargement; in the kidney, associated with those other changes which concur with it to constitute the scrofulous form of Bright's disease; in the arteries effecting their atheromatous degeneration, weakening their coats, and disposing them to aneurism or to rupture. In the liver, this fatty deposition occurs as a very simple change, merely increasing (though often to an immense extent) that molecular oily ingredient, which is native to the endothelium of the gland, and which is visible microscopically in the form of minute globules of oil within the limits of each nucleated cell. In the artery, other changes are associated with the fatty accumulation—at least, whenever it is extreme; changes which have suggested to pathologists a suspicion that the atheromatous degeneration consists in the fatty conversion of some fibrinous or fibriniform material previously deposited there. In the kidney, likewise, at least in that of the human subject, the process of fatty accumulation (shown, as you look on the section of the gland, by a vast number of bran-like spots diffused usually over a pale and flabby surface) is far from being a simple one: you never see the kidney extensively affected in this way, without observing simultaneously that there exists some considerable destruction of tubules, or some serous or fibrinous infiltration of the gland, for which the presence of fat in its endothelium would be insufficient to account. That fatty accumulation in the kidney, confined to its endothelium, and existing there as a primary deposit, constitutes the proximate cause of the scrofulous form of Bright's disease, is a view which I cannot consider supported by conclusive evidence. Many of the microscopical forms (some derived from the tubules during life, others from sections of the organ after death) which have been ascribed to fatty engorgement of the endothelium, are indistinguishable from the *glomeruli* which arise in fibrinous and bloody effusions, and which (as in the white substance of the brain) are often met with in parts not naturally possessing a nucleated cell-growth.

In the domestic cat—at least in our metropolitan cats—the tubules of of the kidney almost invariably (though I presume abnormally) contain a very large quantity of oil; and I think it probable that this quantity may artificially be increased by interference with the locomotion and respiration of the animal. This is a condition of simple fatty accumulation, analogous probably to the fatty liver of the human subject. Though immeasurably greater in degree than any similar accumulation ever observed in the human kidney, it is attended by no destruction of the tubules; nor does it often, if ever, interfere with the function of the organ or with the health of the animal. Some years ago, too hastily, I believed that these kidneys were exquisite analogies of our scrofulous form of Bright's disease, and would explain its human pathology: but further investigation has convinced me of the inaccuracy of this first impression.

Let me now, in a few paragraphs, collect for you the total result of our present knowledge of tubercle, and state to you what inferences may be drawn as to its true pathology from those extensive generalisations which I have set before you.

The material products of the scrofulous diathesis consist in some misdeveloped proteinous ingredient of the lymph and blood. The essence of this mis-development lies in the fibriniform solidification and concretion of something which should remain fluid in the plasma of the blood. I call it fibriniform, because—though it is not identical with fibrin, it probably arises in some analogous method of formation, and undergoes similar final metamorphoses. This solidification occurs in the ascensive development of the lymph and blood; it occurs with an infinite preference in the lymph-glands and in the lungs;—in the former, where lymph is brought into intimate relations with arterial blood; in the latter, where lymph (which is constantly accruing to the blood) first comes into immediate relations with the atmosphere. It seems then to affect the total blood, only by reason of the lymph which is contained in it; in other words, not to be a disease of the total blood, but one of the lymph or nascent blood. Precipitability by the atmosphere is the characteristic of this morbid product; its places of election are determined by this peculiarity: tubercles form, where lymph and blood get their first opportunities of increased oxidation. In accordance with this law, venosity of the blood precludes the tendency to tubercular deposition. The fibriniform product of scrofula is insusceptible of development: it is a dead concretion. Like true fibrin, it may become the seat of calcareous deposit, and may thus form a permanent inorganic concretion. Like true fibrin, it chiefly tends to soften, and its softened substance is copiously infiltrated with fat: probably it undergoes a fatty degeneration. The diathesis in which tubercular deposits occur, is accompanied by a remarkable tendency to the accumulation of fat in the liver, and to the fatty degeneration —either of the substance of other organs, or of some previous infiltration in those organs: possibly this tendency may indicate that the fibriniform material of tubercle in its molecular state can undergo, either in the blood, or in those organs, the same degenerative changes as affect it in its concrete condition. In short, the scrofulous diathesis consists in an inherited peculiarity of blood-development, under the influence of which the nascent blood tends to a molecular death by superoxidation; partly it may appear that these dead proteinous elements can undergo, within the stream of the circulation, such degenerative changes as will qualify them for discharge by excreting organs; partly it may appear that these changes lead to fatty accumulations in the endothelium or in the parenchymatous blastema of such organs; but mainly and characteristically, it is the way of those dead proteinous elements to concrete in the organs where their precipitation has been determined, and there to construct the fibriniform masses called *tubercle*. These masses undergo changes of deliquescence and destruction, which tend to their ultimate expulsion from the system, but which as they entail the disorganization of all surrounding textures by inflammatory and ulcerative processes, are commonly defeated in this tendency when they occur in vital organs (as the lung) or attain it by the complete sacrifice of the invaded organ, as when present in lymph-glands, testicle, or breast.

I have told you that cancer and tubercle are incompatible diseases—that the one excludes the other. You cannot wonder at this. They are pathological antitheses in regard of the blood. In the one case, if I may use so strong a phrase, the blood dies still-born; it never attains its maturity of growth or function; its stops short of the distributive arteries of the body; it never reaches the aorta; it perishes and decays on the threshold of the circulation. In the contrary case of cancer, there is an obstinate excess of vitality, which will not be quenched. You remember how the blood's plasma, as though out of a luxuriance of life, contributed – not to perishable concretions, but to profuse living growths; you remember how these growths, tending to an effort of elimination, still maintained an uninterrupted dependence on the blood, constituting the strange paradox of an organized excretion; you remember how they evolved themselves with exhaustive rapidity out of a too fructifying blastema, vegetating without limit at the expense of other organs, till the whole fluid of the circulation seems to devote itself, away from its slower and legitimate uses, to this impetuous by-play of organization.

Our therapeutical knowledge of tubercle is scanty in the extreme. We are not sure that we possess a drug capable of interfering with its deposit. Iron, which acts so powerfully on the corpuscular development of the blood, and which must therefore, in some degree, effect its total progressive changes, has acquired no reputation for the prevention of tubercular concretions. Cod-liver oil, which is said to influence materially the earlier stages of blood development, has latterly been much vaunted as a contra-scrofulous remedy; its credit, in many cases, is well deserved, and its alleged action on the blood would seem to bring its method of operation nearer to the root of the disease; but much in all this is conjectural, and I dare only speak of it as a matter for continued observation, whether the drug does really affect the initiative acts of blood-development, and whether it does really counteract the tendency to tubercular precipitation. Of the iodide of potassium we know nothing at all: and we have only some very crude and general analogies to guide us to an impression, that its action would rather consist in disintegrating and removing the morbid product, than in hindering its first formation.

Climate, as I have already told you, has a most material influence for and against the development of tubercle. A degree of scrofulous diathesis, which would tuberculize the lungs of a patient in England, would leave him quite unscathed in Cairo, or at the Cape; and, conversely, a minor degree of the diathesis, which would be harmless in a tropical climate, would determine the development of phthisis, if its subject should be made an inmate of our colder atmosphere. I cannot refrain from connecting these notorious facts with that which I have already told you, as to the anti-tubercular power of an increased venosity of the blood. The first and most essential change, wrought by the transference of an animal from England to India, is in the respiratory function, and, consequently, in the oxidation of the blood. Where the thermometer stands at 90°, the quantity of respiration (owing to the rarefaction of the air) must be very much less than where the thermometer stands at 45°; the blood will, consequently, be in a condition of relative venosity, and the disposition to tubercular precipitation will be *pro tanto* diminished. This, I have no doubt, is the manner in which transference to a warmer climate acts beneficially in the *prevention* of tubercular disease. More than this,—where the deposits already exist, or where the ulterior destructive changes have commenced in them, a warm and equal atmosphere can exert a palliative tendency, in diminishing the liability to those catarrhal complications which are so frequent in our precarious climate. Do not believe, in regard of your scrofulous patients, that climate will prevent the softening of tubercular masses which already exist, or that it will ensure the cicatrization of cavities already formed, or about to be formed. Its power is much more limited than this: it will counteract very powerfully the disposition to further deposits: it will give nature more chance of effecting the discharge of softened tubercle in her own gradual manner, than if the process were to be complicated with constant renewals of catarrhal inflammation; and in both these respects—provided there be no large extent of vital organs diseased—it will give powerful assistance towards the patient's recovery. More than this it will fail to accomplish.—*Lancet, August* 3, 1850, *p.* 137.

34.—SPECIAL RULES TO BE FOLLOWED DURING AUSCULTATION OF THE PULMONARY ORGANS.

By Prof. Bennett.

[Professor Bennett, in his Clinical Reports, remarks:]

1. In listening to the sounds produced by the action of the lungs, we should pay attention to three things,—1st, The natural respiration; 2nd, The forced or exaggerated respiration; and, 3rd, The vocal resonance. For this purpose, having listened to the sounds during ordinary breathing, we direct the patient to take a deep breath, and then, still listening, we ask him a question, and during his reply judge of the vocal resonance.

2. You should commence the examination immediately under the centre of one clavicle,—and having ascertained the nature of the sounds and vocal resonance there, you should immediately listen in exactly the corresponding spot on the opposite side. The examination should be continued alternately from one side to the other, in corresponding places, until the whole anterior surface of the chest is explored. The posterior surface is then to be examined in like manner.

3. When, in the course of the examination, any thing different from the normal condition is discovered at a particular place, that place and the parts adjacent should be made the subject of special examination, until all the facts regarding the lesion be ascertained.

4. It is occasionally useful to tell the patient to cough, in which case we are enabled to judge, 1st, Of forced inspiration, as it precedes the cough; and, 2nd, Of the resonance which the cough itself occasions.

Of the Sounds elicited by the Pulmonary Organs in health and in disease.—I am anxious to impress upon you, that the sounds which may be heard in the lungs are like nothing but themselves. Students are too apt to take up erroneous notions from reading on this subject, and, instead of listening to the sound actually produced, fatigue themselves in a vain endeavour to hear something like the crackling of salt, the rubbing of hair, foaming of beer, or other noises to which these sounds have been likened. Preconceived notions frequently oppose themselves to learning the truth, and have to be got rid of before the real state of matters can he ascertained. Hence the great importance of obtaining your first impressions of the sounds to be heard by auscultation, not from books or lectures, but from the living body itself.

If you listen through your stethoscope, placed over the larynx and trachea of a healthy man, you will hear two noises, one accompanying the act of inspiration, and the other that of expiration. These are called the *laryngeal and tracheal sounds or murmurs.* If you next place your stethoscope a little to the right or left of the manubrium of the sternum, you will hear the same sounds diminished in intensity. These are the *bronchial sounds or murmurs.* If now you listen under and outside the nipple on the right side, or posteriorly over the inferior lobe of either lung, you will hear two very fine murmurs. That accompanying the inspiration is much more distinct than that accompanying the expiration. By some, on account of its excessive fineness, it is stated that there is no expiratory murmur in health; but this is incorrect. These sounds, then, are the *vesicular respiratory murmurs.* All these sounds become exaggerated during forced respiration, but in a state of health they never lose their soft character. Again, if you listen in the same places, whilst the individual speaks, you will hear a peculiar resonance of the voice, which has been called, in the first situation, *pectoriloquy;* in the second, *bronchophony;* while in the third, it is scarcely audible. A knowledge of these circumstances, and a capability of appreciating these sounds, are necessary preliminary steps to the right comprehension and detection of the murmurs which may be heard during disease.

I have to suppose, then, that you have made your ears familiar with these sounds, and that you are acquainted with the present state of theory regarding their formation. This last may be stated in very few words to be, that the respiratory murmurs are occasioned by the vibration of the tubes through which the air rushes, according to well-known acoustic principles. Hence they are loudest in the trachea, finer in the large bronchi, and finest in their ultimate ramifications. The vocal resonance, on the other hand, originates in the larynx; and diminishes or increases,—1st, according to the distance of any point from the source of the sound; and 2d, according to the power which the textures have in propagating it.

If now you examine, in succession, any six of the cases in the wards which are labouring under well-marked pulmonary diseases, you will have no dificulty in recognising that all the sounds you hear may be classified into two divisions,—1st, alterations of the natural sounds; 2d, new or abnormal sounds, never heard during health.

I. *Alterations of the Natural Sounds.*—All the sounds of which we have spoken, which can be heard in the lungs during health, may, in certain diseased conditions, be increased, diminished, or absent; their character or position may be changed; and, with regard to the respiratory murmurs, they may present alterations in rhythm, or duration with respect to each other.

Alterations in Intensity.—Some persons have naturally louder respiratory murmurs than others; if this occur uniformly in both sides, it is a healthy condition. Occasionally, however, the sounds are evidently stronger in one place, or on one side (*puerile respiration*), generally indicating increased action of the lung, supplementary to diminished action in some other part. In the same manner, there may be feeble respiration simply from diminished action, as in feeble or old persons, but it may also be occasioned by pleurodynia, obstructions in the larynx; trachea, or bronchi,—pleurisy, or pulmonary emphysema, or exudations filling up a greater or less number of the air-cells and smaller tubes, as in pneumonia, phthisis, &c. Complete absence of respiration occurs when there is extensive pleuritic effusion of hydrothorax.

Alterations in Character.—The various respiratory murmurs may, in certain conditions of the lung, assume a peculiar harshness, which, to the ear of the practised auscultator, is a valuable sign, indicative of altered texture. Thus in incipient phthisis the vesicular murmur under the clavicle is often *rude* or *harsh.* In pneumonia the bronchial respiratory murmur presents a similar character. When ulceration exists, it becomes what is called *cavernous* (hoarse or blowing); and in certain cases of pneumothorax with pulmonary fistula, it assumes an *amphoric* character.

Alterations in Position.—It frequently happens that the sounds which are natural to certain parts of the chest, heard distinctly where in health they are never detected. Thus, in pneumonia, *bronchial* or *tubular breathing*, as it is sometimes called, may be evident, where only a vesicular murmur ought to exist. This is often well marked with regard to the vocal resonance, as certain lesions, which occasion condensation or ulceration, will enable us to hear in parts where, under ordinary circumstances, no voice can be heard, either bronchophony or pectoriloquy.

Alterations in Rhythm.—In health, the inspiration is usually three times as long as the expiration. In certain diseased conditions this relation is altered, or even inverted. In incipient phthisis we often find the expiration unnaturally prolonged. In chronic bronchitis and emphysema it is three or four times longer than the inspiration.

II. *New or Abnormal Sounds.*—These are of three kinds; 1st, rubbing or friction noises; 2d, moist rattles; 3d, vibrating murmurs.

1. *Rubbing or Friction Noises* are caused in the pulmonary apparatus by some morbid change in the pleuræ, whereby, instead of sliding noiselessly on one another, they emit a rubbing sound. This may be so fine as to resemble the rustling of the softest silk, or so coarse as to sound like the creaking of a saddle, grating, rasping, &c.; and between these two extremes you may have every intermediate shade of friction noise. This variation in sound is dependent on the nature of the alteration which the pleuræ have undergone. If covered with a softened thin exudation, the murmur will be soft; if it be tougher and thicker, the sound will be louder; if hard, dense, and rough, it will assume a creaking, harsh, or grating character, &c. &c. These noises are heard in the various forms of pleurisy.

2. *Moist Rattles* are produced by bubbles of air traversing or breaking in a somewhat viscous fluid. This may occur in the bronchi, when they contain liquid exudation, mucus, or pus, or in ulcers of various sizes. They may be so fine as to be scarcely audible (when they have been called *crepitating*), or so coarse as to resemble gurgling or splashing, when they have received the name of *cavernous*. Here again, between these two extremes, we may have every kind of gradation, to which auscultators have attached names, such as *mucous, submucous, subcrepitating*, &c. &c. With these names you need not trouble yourselves; all that it is important for you to recognise is, that the sound be *moist*, and you will easily recognise that the rattles are coarse or large, in proportion to the size of the tubes or ulcers in which they are produced. These rattles may be heard in pneumonia, phthisis pulmonalis, bronchitis, pulmonary apoplexy, &c. &c.

3. *Dry Vibrating Murmurs* arise when the air-tubes are obstructed, constricted, or lose their elasticity and become enlarged, whereby the vibrations into which they are thrown by the column of air, produce sounds or tones of an abnormal character. Hence murmurs may be occasioned of a fine squeaking (*sibilous murmur*), or of a hoarse snoring

character (*sonorous murmur*), and between the two extremes, there may be all kinds of variations, to which ingenious people have applied names. These only cause confusion; all that is necessary, being to ascertain that the murmur is *dry*, and you will readily understand that the fineness or coarseness of the sound will depend on the calibre of the tube or cavity thrown into vibrations. They are usually heard in cases of bronchitis and emphysema. Occasionally they present a blowing character, as when ulcers are dry, which often occurs in phthisis.

The *vocal resonance*, besides undergoing the changes already noticed in intensity, character, and position, may give rise to abnormal sounds. Occasionally it presents a soft reverberating or trembling noise, like the bleating of a goat (œgophony). The value of this sign, as indicative of pleurisy, was much overrated by Laënnec. At present it is little esteemed. Sometimes the resonance gives rise to a metallic noise, like dropping a shot into a large metallic basin, or the note produced by rubbing a wet finger round the edge of a tumbler or glass vessel. This is often best heard immediately after a cough, in cases of pneumothorax, or large tubercular excavations of the lung. Œgophony is supposed to be produced, when a thin layer of serous fluid between the pleuræ is thrown into vibrations. The cause of metallic tinkling has created great discussion; but Drs. Spital and Skoda have shown that the existence of air in a cavity which is thrown into vibrations is the necessary condition.

Such, then, are the principal sounds which may be heard by auscultation of the pulmonary organs in health and during disease. Many writers have endeavoured to point out their diagnostic importance, and drawn up rules which have always appeared to me much too arbitrary. Indeed, in so far as the education of medical students is concerned, I have long been persuaded that the study of these rules has retarded their powers of diagnosis, and afterwards led to dangerous errors in practice. I know of no dogma, for instance, more mischievous than the one which asserts a crepitating (that is a fine moist) rattle to be pathognomonic of pneumonia, because it is just as common in phthisis, and is frequently heard in various other lesions of the pulmonary organs. Hence, we should regard a crepitating rattle, not as indicative of this or that so-called disease, but simply of fluid in the smaller air-passages; increased resonance of the voice, as indicating hollow spaces with vibrating râles, or increased induration of the pulmonary textures, and not as diagnostic of phthisis, pneumonia, &c., and so on. I wish, then, strongly to impress upon you,—

1st, That the different sounds are only indicative of certain physical conditions of the lung, and in themselves bear no fixed relation to the so-called diseases of systematic writers.

2nd, No single acoustic sign, or combination of signs, is invariably pathognomonic of any certain pathological state,—and conversely, there is no pathological state which is invariably accompanied by any series of physical signs.

3rd, Auscultation is only *one* of the means whereby we can arrive at a just diagnosis, and should never be depended on alone.—*Monthly Jour. of Med. Science, Nov.* 1850, *p.* 444.

35.—ON THE PATHOLOGICAL STATES OF THE LUNG CONNECTED WITH BRONCHITIS AND BRONCHIAL OBSTRUCTION.

By Dr. W. T. Gairdner, Pathologist to the Royal Infirmary of Edinburgh.

[Dividing his subject into two parts, Dr. Gairdner discusses in the first, the effects which follow accumulation of mucus and inflammatory products in the bronchial tubes, causing serious obstruction; and in the second, the secondary and more permanent disorganizations of the pulmonary texture which result from the former under peculiar circumstances. The characters of the secretion from disordered mucous membrane vary very greatly. In the earlier stages the mucus is profuse, thin, watery, and frothy, mixed with air-bubbles of all sizes. At a later period the mucus has become yellowish and mixed with pus corpuscles, and occasionally it is composed of pus nearly pure, but always viscid and thick; but in these cases the pulmonary tissue is rarely unaffected.]

Whan the disease has been of considerable duration, and has nevertheless been the result of an acute attack, there are usually found, on incising the bronchi, numerous yellowish pellets, or irregular flocculent masses of somewhat curdy consistence, which float in the more recent and thinner mucus, without in any degree becoming amalgamated with it. These masses are found of all sizes, and constitute a well-known form of the expectoration of bronchitis in its advanced stages. They are no doubt formed by the evaporation of the more fluid parts of the mucus, in consequence of the constant passage over it of dry air; the inspissated material clinging to the walls of the air passages, until detached and washed away by renewed exudation from the mucous membrane. Such a mass is not unfrequently seen plugging the whole calibre of one of the larger or smaller bronchi, in such a position as to leave no doubt that in the act of inspiration it must have acted the part of a ball-valve, completely preventing the access of air to the part of the lung involved, by falling back upon the orifices of the smaller bronchi, into which its size would prevent it from entering.

When bronchitis, or bronchial catarrh, has existed for some time in a comparatively slight form, and with the expectoration only of an increased quantity of mucus unmingled with pus, portions of this secretion become inspissated in the form of a thick, glairy, tenacious, semi-transparent material, sometimes resembling raw white of egg. In one or two cases I have pressed out of the smaller bronchi plugs of altered mucus of a still more tenacious character, like cold glue, and nearly, if not quite, transparent. But this amount of inspissation is not common.

The effects of these morbid accumulations in producing obstruction of the bronchial tubes are well known to the auscultator. So long as the mucus is thin and watery, or even more or less purulent, there is no serious or complete impediment to the passage of air; which, as the fine and coarse mucus râles, accompanied by vesicular respiration, indicate, finds its way through the fluid to the ultimate bronchial and pulmonary vesicles. The fluids, too, at this stage, move freely throughout the bronchial tree even to its minutest branches, and when in excessive quantity, are readily expectorated in the act of coughing. This stage of bronchitis, therefore, is comparatively little apt to be accompanied by urgent dyspnœa, or by changes in the condition of the pulmonary texture.

It is otherwise, however, when after a time the secretions within the bronchial tubes have become inspissated; the mucus having either become purulent and formed itself into the tenacious pellets above-mentioned, or assumed the stringy, tenacious consistence proper to the more chronic forms of the disease. When, under these circumstances, expectoration is hindered, either by the tenacity of the mucus itself, the weakness of the patient, or any other cause, the tubes become really obstructed, the sound of vesicular respiration is at some points of the lung much diminished, or it may be, altogether lost, and the bronchial râles are correspondingly modified, indicating the passage of a smaller quantity of air with a much greater amount of resistance; the ordinary mucus rattles being supplanted in part by the sharp "clicking" and valve-like sounds so well described by Dr. Williams.

It is quite evident to the observer of a patient in this condition, or, indeed, in any stage of bronchitis attended with laborious breathing, that the chief difficulty is invariably in *inspiration*. This act is accomplished only with the aid of all the accessory muscles; and, even then, the contracted state of the chest, especially in children, and the imperfect descent of the diaphragm, show that in proportion to the power employed, the amount of air entering is small. The expiratory act, on the contrary, is always accomplished with comparative ease; and any serious obstruction to the performance of this act is at once got rid of, or dislodged, by the additional impulse given to the expired air in coughing or hawking. These phenomena of laborious breathing, particularly the long-drawn, exhausting, inadequate inspiration, are probably quite peculiar to obstructive bronchitis. They occur, it is true, in diseases of the heart; but in them, I believe, only secondarily, from the accumulation of mucus in the respiratory passages. The dyspnœa of *pure* pneumonia, on the other hand, is something quite different, being a mere *acceleration* of the respiration without any of the heaving or straining inspiration observed in bronchitis, or in cases where the two diseases are combined. So much is this the case, that I have repeatedly observed patients affected with a great extent of pneumonia in both lungs, and in whom the extreme lividity, and the respirations numbering 50 or 60 in the minute, showed infallibly the amount to which the function of the lung was interfered with: and who nevertheless lay quietly in bed, breathing without any of the violent effort, or the disposition to assume the erect posture, so constantly accompanying the more dangerous forms of bronchitis. If this freedom from orthopnœa and laborious breathing be not uniformly characteristic of true pneumonia, it is because that disease comparatively seldom exists, as is well known, uncomplicated by some degree of bronchial affection.

The cause of the inadequate and laborious inspiration in bronchitis while the expiration is comparatively easy, is to be found, I believe, not merely in the smaller power of the inspiratory muscles to dilate the chest, nor in the advantage which the expiratory forces derive in the dislodgment of obstructive mucus from the sudden impulse of coughing, but in the mechanical relations of the tenacious inspissated mucus to the calibre of the bronchial tubes.

There can be, I think, little doubt that many of the paroxysmal accessions of dyspnœa in persons affected with bronchitis are due to accidental change of position of the pellets and ropy masses of inspissated mucus, which accumulate in the tubes. At least stethoscopic examination frequently reveals the signs of obstruction in particular parts of the lung, supervening rapidly, and disappearing again with equal suddenness, in consequence of the accession of cough.

[The condition of the pulmonary texture springing more directly than any other from obstruction of the bronchi, by mucus of a certain degree of tenacity, Dr. Gairdner is led to believe is collapse of the air-cells. He states, that during the epidemic fever which prevailed in Edinburgh in 1847, it was frequently observed that the lungs in persons of all ages were the seat of a peculiar form of condensation, the section being smooth, not granular; consistence somewhat tough, not breaking down under the finger. These appearances occurred very commonly in typhus fever, or in the abdominal form, commonly known as "typhoid," accompanied by intestinal ulceration. Dr. Gairdner says that the result of his observations was, that this state was not connected with any form of morbid deposit, but was, in its purest form, a condition of imperfect expansion or collapse of the pulmonary tissue, similar to that described in the lungs of infants, under the name of atelectasis. He says,]

Observations made in the years 1848-9 left me no longer in doubt, that the lungs of adults not unfrequently display portions more or less limited or diffused, which are so perfectly condensed as to sink rapidly in water, and yet differ widely, in appearance and microscopic character, from truly inflamed lung, as well as from all the atrophic conditions of the organ which can be clearly traced to inflammation or structural disease as their cause. In some instances, such portions presented exactly the appearance of fœtal lung; and only the manifest absurdity of the proposition could have prevented the observer from ascribing their state to the same cause as the congenital non-expansion of the lung, or atelectasis of Jörg. In the absence, therefore, of any manifest explanation of the compression or collapse of the tissue observed in these cases, I was obliged, provisionally, to rest satisfied with the knowledge of the fact that most of the lobular and many of the more diffused forms of condensation usually ascribed to pneumonia, in the adult as well as the child, were really the result of some other and unknown condition.

Some time after this collapsed condition of the lung had become tolerably familiar to me, the following case occurred, presenting a marked example of the coincidence of the affection with obstructed bronchi, *both lesions being limited to a very small and circumscribed space in the lung.*

Case I.—*Lobular Collapse of Lung—Obstructed Bronchi.*—A man, æt. 18, affected with necrosis of the femur, anasarca, and some degree of abdominal dropsy from disease of the liver, spleen, and kidneys, died in the surgical hospital in the last stage of exhaustion and emaciation. No pulmonary symptoms had attracted notice.

The lungs were generally normal in appearance. At one or two places, however, they crepitated imperfectly over spaces not larger than an inch in diameter; these portions were quite circumscribed by abrupt margins; the bronchi leading to them yielded on pressure a very tough gelatinous mucus (like thick calves'-foot jelly), which contained only ciliated epithelium, and had otherwise the ordinary appearance of mucus under the microscope.

In this case there was little or no room for fallacy in judging of the connection of the pulmonary collapse with bronchial obstruction. The parts affected, with the bronchi leading to them, were quite capable of being isolated; and there was no trace of an inflammatory affection in any part of the pulmonary tissue. The co-existence of the collapse with bronchial obstruction thus accurately limited, could only, in all probability, be explained by the dependence of the one upon the other as its cause The following case, however, which occurred during the present year, is still more conclusive.

Case II.—*Extensive Collapse of Lung—Obstruction of Bronchi by a Tubular Membrane.*—A girl, aged about 21, was attacked, after a surgical operation upon the tongue, with urgent dyspnœa having the character of a laryngeal affection. Tracheotomy was performed, but failed to save her. She died about twenty-four hours afterwards.

Dissection performed Feb. 22, 1850. The appearances in the air-passages and lungs, were the following:—

The right lip of the glottis was infiltrated with serum, which distended the epiglottidean fold of mucous membrane so much as nearly to close the opening. The larynx and trachea were occupied by a tubular false membrane throughout their whole length; it was about a line in thickness, friable, and of a yellowish white colour. This membrane was continued at the bifurcation into the right bronchus, but the left was free from it. It could be traced throughout the bronchi of the right lung even into the minuter ramifications, in which it assumed the form of a very soft opaque matter, like a thick emulsion. On examination with the microscope, the membrane presented the usual appearance of coagulated fibrin, with some pus or mucus corpuscles, but these not in very large numbers.

Both lungs were more collapsed, and contained less air than natural. The left, however, was much less affected than the right. The latter was at some parts completely flaccid and free from air, while others presented an imperfect crepitation. The upper lobe crepitated more than the others, and its colour was mostly natural; but in its lower portions it approached more in colour to the middle lobe. This was perfectly non-crepitant, of a grey colour, having a smooth flesh-like section, and infiltrated with a thin serosity, having a faint reddish tinge, nearly transparent, and presenting under the microscope only epithelium, pigment cells, and a very small amount of pus cells, and granular matter. The lower lobe was not quite so much compressed, and contained a good deal of blood. The bronchial glands were slightly enlarged, and the whole pulmonary mucous membrane highly vascular.

In this case, as in the former, there were no pneumonic appearances; for the small amount of pus cells which existed in the serosity scraped from the lung, were probably derived from the small bronchi. and were, at any rate, quite inadequate to account for so remarkable a consolidation. Neither can it be supposed that there was any older structural lesion of the pulmonary substance, as the girl was undoubtedly in good general health at the period of her admission into the hospital, and the whole stages of the fatal disease were too rapid to admit of any complicated structural change. The collapsed lung presented exactly the appearances so commonly seen in organs compressed by pleuritic effusion, except that the middle lobe had rather more of an œdematous appearance than is common under such circumstances. Here, then, is an instance of collapse of the entire lower lobes and part of the upper in the right lung, as complete as it could have been from external pressure, and coinciding with a manifest cause of obstruction in the whole of the bronchi of that lung; whereas on the opposite side, where the obstruction had been much less considerable, the lung was comparatively expanded and normal. It is impossible not to see here a relation of cause and effect; and as it will not be maintained that the deposit of false membrane was the result of the collapse, the converse proposition is, I think, scarcely to be avoided.

The form of bronchial obstruction in this case is a sufficiently rare one in the adult, but by no means unexampled. It is not at all clear whether the membrane had never formed in the bronchi of the opposite lung, or had been dislodged by expectoration. In the former case it is not easy to understand the non-symmetrical character of the fibrinous exudation, while the mucous membrane appeared equally inflamed on both sides of the chest; in the latter it is still more incomprehensible, how the complete expectoration of an extensively ramified membrane could take place, without being observed during life. But of the fact of its presence on the right, and its entire absence on the left side, there is no doubt.

The observation of these two cases, and the conclusions which appeared naturally to follow from them, as to the connection between collapse of the air cells and bronchial obstruction, tended to throw light on many of the obscurities of the preceding observations. More particularly, it seemed to me that they furnished the connecting link necessary for the explanation of the peculiar forms of "lobular pneumonia" (as they were generally considered), which had frequently occurred both in the lungs of children and adults. The peculiar connection of lobular condensation with bronchitis was a matter of common observation with me before this period; the more recent literature on the diseases of the lung in children also seemed to point to the same fact; and it seemed every way probable that many of the more complex phenomena of pulmonary disease, would receive explanation from the more careful consideration of their connection with obstructed bronchial tubes. I therefore renewed my observations with great interest, directing them particularly, and with more care than formerly, to the relative condition of the bronchial tubes and pulmonary tissue, in all cases where either pulmonary collapse or bronchitis existed to any considerable extent. The general result to which I have been led is, 1st, that in all cases of collapse of the lung not caused by external pressure, the bronchi have presented unequivocal appearances of obstruction; 2d, that in most, if not all, the instances of severe and fatal bronchitis, especially if the secretions had become ropy or inspissated, more or less collapse of the pulmonary texture has also been present; 3rd, that under peculiar circumstances a much less amount of obstruction may be attended with collapse of the pulmonary texture, the symptoms in such cases probably attracting little attention.

In maintaining (as I have no hesitation in doing) that some degree of collapse of the lung is an almost invariable concomitant of bronchitis of a certain degree of intensity, it must not by any means be supposed that *complete* loss of crepitation is to be looked for in any part of the tissue in the majority of cases. The usual fact is, that the collapse is in the incomplete and diffused form; but I believe, nevertheless, that dulness of percussion during life from this cause, and complete lobular or diffused collapse after death, especially in the posterior parts of the organ, will be found to be much more common in the bronchitis, as well of adults as of children, than is commonly supposed. Of the truth of these opinions, both recorded observations, and unrecorded recollections, appear to me to furnish no inconsiderable amount of evidence.

[On the opinions of Dr. West upon this subject, the author observes:]

The descriptions of Dr. West well deserve to be read, on account of their clearness and accuracy of detail; and his views as to the frequent connection of the collapse of the lung with bronchitis, and its dependence in many cases, on that affection, harmonize so closely with my own, that I can scarcely regret the late period at which I became acquainted with them, as it enables me to point out more clearly the identity of the conditions in the adult with those in the child. "In the child," says Dr. West, "nothing more is needed than a copious secretion of mucus into the bronchi, or a feeble condition of the vital powers, to prevent the air from freely entering the pulmonary vesicles, and thus to induce the collapse of a large portion of the lung."

[Having endeavoured to show that among the pathological phenomena connected with obstruction of the bronchi with tough mucus, or other inflammatory products, one of the most frequent and important is collapse, to a greater or less extent, of the air vesicles, to which the obstructed bronchi lead, Dr. Gairdner then proposes to show the probable dependence of many important chronic alterations of the pulmonary texture on the condition of collapse from bronchial obstruction. He first notices a condition which occurs so frequently in connection with it, that he considers it a secondary consequence of bronchitis, accompanied by pulmonary collapse.]

Bronchial abscess.—It not unfrequently happens that, in the centre of the collapsed lobules of a lung affected with acute bronchitis, there are found small collections of pus, varying in size from that of a hemp-seed upwards to double or treble that volume. These small abscesses present, on section, an appearance so like that of softening tubercles, as to be very readily mistaken by many persons for these bodies; and the resemblance is all the greater on account of the peculiar limited form of the condensation by which they are generally surrounded, which when felt by the touch from the exterior of the lung, is exceedingly deceptive. In their interior, however, these little abscesses contain, in the recent state, a very fluid pus; moreover, they are often met with as acute lesions produced by a few days of illness, and without a trace of tubercle in any other organ. This is peculiarly characteristic in the young child, in which tubercle of the lung so very rarely occurs without extensive deposits in the bronchial glands, whereas the present lesion is accompanied in its pure form by nothing more than slight enlargement. When the pus contained in these abscesses, in their recent form, is pressed or scraped out, they are seen to be lined with a fine villous false membrane, very different from the thick curdy mass which generally surrounds softened tubercles; in others they are not abruptly limited at all, the pus appearing to lie in contact with the surrounding pulmonary tissues. When the bronchi leading to the lung so affected are carefully incised, they are found much inflamed; the mucous membrane vascular, thickened, and covered with pus; and some of the bronchi thus affected can be observed to communicate with these purulent collections, the mucous membrane having evidently been, at the point of communication, destroyed by ulceration, and either stopping short abruptly, or becoming gradually incorporated with the false membrane lining the abscess. Sometimes these abscesses are found to break into one another, and form more considerable excavations; in one instance I found them connected with a gangrenous condition; more commonly, however, they remain of limited size, preserving perfectly the direction and relations of the bronchial tubes.

These abscesses occur in the diffused as well as in the lobular form of condensation from collapse; and both forms may sometimes be seen in the same lung. A similar lesion may accompany true pneumonia, but always in those cases where it is combined with intense bronchial inflammation. In this case, also, it tends to the formation of more considerable and more irregular excavations.

Such cavities have been pretty accurately described by Barthez and Rilliet (as well as by other French writers on the bronchitis and pneumonia of children), under the name of "*vacuoles.*" They are, however, far from uncommon in adults, though, perhaps, more frequent in the so-called "lobular pneumonia" of children. As these "vacuoles" unquestionably arise from the accumulation of pus primarily in the extreme bronchial tubes of the collapsed lobules, the name of bronchial abscesses may serve to distinguish them from other forms of excavation of the lung.

That Laënnec should have overlooked these small excavations is singular enough. Probably he may have considered them a form of tubercle, a term which has been used by him and others with sufficient vagueness to cover a multitude of anomalous lesions. His assertion that in many hundreds of pneumonic lungs, he had only met with collections of pus five or six times, can only be explained upon this principle. It is, indeed, quite true that in simple pneumonia there is little tendency to the formation of abscess, the pus escaping rapidly by the pervious bronchi; but as we have seen that Laennec has probably confounded the pneumonic with the bronchitic condensation, I think it also probable that he has not distinguished the bronchial from the tubercular abscess, especially as these lesions often resemble each other so much as to have led me habitually, for some time to call the former *tuberculoid disease* of the lung.

In the child, owing to causes which will be presently mentioned, collapse, in its most complete form, very readily occurs under all circumstances, as a consequence of acute bronchitic attacks. In commenting upon this peculiarity, Dr. West, whose opinion on this subject I have before referred to, remarks that it is by the collapse of the lung "that we must explain many of the instances in which urgent dyspnœa, and all the symptoms of serious pulmonary disease, have developed themselves in the course of a few hours out of what had seemed to be a severe cold, or a bronchitis of moderate intensity. This, too, accounts for the occasional supervention of dulness on percussion, and of bronchial respiration in the child; so that you may discover them in the morning in a situation where, overnight, the percussion was good, and no sound of graver import than large crepitation." To this remark I would only add, that dulness on percussion supervening under such circumstances is not necessarily attended with any increase of fever; and that careful attention to the state of the patient in this respect may be in some cases the only mode of forming an opinion with any degree of accuracy as to the presence or absence of true pneumonia in a case of bronchitis. Of this fact a striking instance occurred to me lately in the case of a little boy, three years old, whom I attended during an attack of acute bronchitis. He was a delicate child, and had suffered several times from bronchitic attacks. His chest presented a good deal of deformity, particularly on the left side, of the kind called "pigeon-breast." On the fourth day of the attack, which had all the usual acute characters during the first three days, decided dulness on percussion, with obscurity of respiration, appeared in the right back; the dyspnœa continuing considerable, but *the fever rather abating than increasing*. Under the influence of an emetic, and a forced decubitus on the opposite side to that on which the dulness existed, almost every trace of it had disappeared in thirty-six hours, and the dyspnœa was entirely relieved.

If these views be correct, they cannot be without practical importance. The rules both of diagnosis and treatment, in regard to bronchitis and its complications, must probably undergo some modifications, in order to be safely followed. Such an axiom as the following, at least, which is one very generally conceived to be unquestionable, will not, I believe, stand the test of renewed observation, if the fact of collapse of the lung from bronchial obstruction be kept in view. "The stroke-sound of the chest," says Dr. Williams, "is not materially impaired by catarrh; and, accordingly, the partial suspension of the breath-sound in a part of the chest in this disease *cannot be mistaken for that caused by hepatization.*" The reader will at once perceive that the clearness of the stroke-sound here alluded to is not always, perhaps not even generally, to be expected under such circumstances; nor can the opposite condition, even when combined with diminished or suppressed respiratory murmur, or with bronchial respiration, be allowed necessarily to indicate hepatization. The judicious physician has doubtless often been saved from error in the application of his remedies in this, as in many other cases where an unsound pathology has prevailed, by adhering to the great principles of our art, and the teaching of personal experience, rather than to receive formulas and rules on matters of detail. But it is difficult to escape from the conviction, that the influence of a name, or the vanity of an *exact* diagnosis, may have misled many into such errors; and that in cases of supposed broncho-pneumonia or typhoid pneumonia, the practice may frequently have embraced blood-letting, heroic antimonials, or calomel and opium, when emetics and expectorants, with suitable stimulation, would have been better adapted to the circumstances of the case.

Since the preceding observations were in type, I have received the recent work of Dr. Fuchs, on the bronchitis of children, which in its elaborate descriptions, both of symptoms and post mortem appearances, furnishes additional evidence of the connection of collapsed lung with bronchitis. The state of the lung, called "lobular pneumonia," by most authors, and "état fœtal" by Legendre, is regarded by Fuchs as a direct consequence of bronchitis; and its relation to bronchial obstruction on the one hand, and to peculiarities in the infantile system on the other, is certainly more clearly stated than by any previous author.

Mechanism, Causes, &c., of Bronchitic Collapse.—The object of the remarks hitherto made, in reference to this subject, has been chiefly to establish the frequent coincidence and probable relation of cause and effect between the obstruction of the bronchi and the collapse of the air-vesicles. The nature and rationale of that connection now fall to be more particularly considered.

When a bronchial tube is in any way obstructed, or much diminished in calibre, at one or more points, the question arises, what is the mechanical effect of the movements of respiration upon the amount of air thus partially imprisoned behind the obstructed part? If the obstruction be complete, of course no change can take place, at least mechanically; but in the case of its being, as it usually is, incomplete, it may be supposed that the inspiratory act tends to draw in more air than the expiration can expel, and consequently that the air tends to *accumulate* in the vesicles; or, *vice-versa*, that the air behind the obstruction tends constantly to *diminish* in amount, owing to the comparative inefficiency of the inspiratory act; or finally, that the forces equalise each other, and the quantity of the enclosed air *remains unaltered.*

Now, it cannot be denied that from the pathological anatomy of bronchitis, a *prima facie* case might be made out for each or all of these theories; for, although only one side of the question has hitherto been brought prominently forward in the preceding part of this paper, the reader will not have failed to remark that in several cases in which bronchitic collapse of the lungs existed, other parts of the same organs were affected with emphysema or dilatation of the air-cells. Indeed, so familiar is this conjunction of emphysema with bronchitis, as to have suggested to Laënnec, long ago, the first of the three theories above mentioned as to the cause of dilatation of the air-vesicles in that disease. Again, fatal cases of bronchitis undoubtedly occur, at least in adults, in which there is no change of the pulmonary texture so marked as to afford support to either of the first two theories.

Laënnec's view of the consequences of obstruction was founded on the idea of the comparative weakness of the expiratory, as compared with the inspiratory, forces. "The mucus secreted into the bronchi, in consequence of pulmonary catarrh, must, especially if it is very viscous, present a great resistance to the free passage of air in inspiration and expiration; and we shall show, in speaking of the râle, that this resistance often goes the length of producing complete, though momentary, obstruction of a part of the bronchial ramifications. Now, as the muscles which subserve inspiration are strong and numerous, while expiration is produced only by the elasticity of the parts, and the weak contractions of the intercostal muscles, it must necessarily happen that the air, which has been forcibly driven beyond the obstruction in inspiration, will not be able to overcome it in expiration, and will be in a manner imprisoned, by a mechanism not unlike that in the butt-end (condenser) of an air-gun." I have given this passage from Laënnec entire, because, notwithstanding the palpable fallacy it contains, it has been referred to, and its conclusion adopted without comment, by almost every systematic writer in this country as well as in France. The fact is, however, that though *ordinary* inspiration is more of a muscular act than *ordinary* expiration (merely because in the latter there is little or no resistance to be overcome, to which the elastic subsidence of the parietes is not adequate), yet the residual effective force for overcoming adventitious obstruction is very considerably greater in expiration. The *forced* or *muscular* expiratory act is, in fact, about one-third more powerful, as measured by its effect upon a pressure-gauge, than the extreme force of inspiration; and it is this force which is thrown into action when obstruction in the tubes is to be overcome. In the act of coughing, moreover, we find a beautiful mechanism, by which the air within the vesicles is discharged outwards at a *maximum amount* of pressure, and brought to bear with all the additional mechanical advantage of a sudden impulse, on every obstructing substance within the bronchial tree,—a cumulative provision which does not exist in the case of the inspiratory force. There can be no great difficulty, therefore, on these grounds, in coming to the conclusion, that the data of Laënnec's hypothesis are quite erroneous, and that the practical efficiency of the expiration in forcing air through obstructions must be, *cæteris paribus*, far greater than that of the inspiration. I have already alluded to the fact, that this is consistent with general experience; for while the inspiratory act is always, in bronchitis of considerable intensity, attended with extreme difficulty, the expiration is never so.

The question of the origin of emphysema of the lung will be considered in the second part of this memoir, in which I shall endeavour to account for its connection with bronchitis, by referring its production to a totally different mechanism from that just mentioned. In the meantime, I would remark that, in order to establish a direct relation between this lesion and bronchial obstruction, it is necessary to show not merely that emphysema occurs in connection with bronchitis, but that it occurs especially or exclusively *in those parts of the bronchitic lung where obstruction can be shown to exist.* This is the proposition which I conceive the preceding pages have tended to establish as regards bronchitic collapse, and on the ground of which I have argued for the relation of cause and effect between this lesion and obstruction. In how far does emphysema fulfil these conditions?

It is well known to every one who has studied the anatomy of this pathological state, that the emphysematous portions of a lung can generally be inflated from the bronchi with the greatest ease. Indeed, so far as my own experience in this matter is concerned, I cannot recal any instance in which the pressure of the air was not found to reach the emphysematous parts with as great rapidity as the rest of the lung. In the collapsed lung, on the contrary, as I have already shown, very considerable resistance is often opposed to its inflation from the bronchi—a resistance only to be overcome by pressure many times greater than can ever occur in the vital act of inspiration. If this observation be correct, it is plain that the emphysematous parts of the lung are usually free, the collapsed parts obstructed.

All doubt, as to the real effect of a solid obstruction in the bronchi on the air in the lung is removed by the direct experiments of Mendelsohn and Traube on animals. The former inserted a leaden shot into the trachea of a dog, pushing it down as far as possible into the bronchus with a probe. In another instance he inserted a ball of paper. In both cases, the parts to which the obstructed bronchi led were red and void of air. In the former there were emphysematous portions in the other parts, and in the opposite lung. Traube's experiments were similar, but more numerous. The general result was, that the artificial obstruction of a bronchus always produced expulsion of the air from the corresponding part of the lung, which had a dark red colour, and presented the characters of collapse.

It is clear, therefore, from experiment, as well as from pathological observation, that the most usual and most direct effect of obstruction, or of diminished calibre of the bronchi, however caused, is not accumulation but diminution in quantity of the air beyond the obstructed point. It is probable that this is due in part to the comparative weakness of the inspiratory power, and that the proposition of Laënnec may, therefore, correctly enough be inverted. There is also, however, another mechanical condition which comes into play in producing collapse from obstruction, especially in the case of a viscid plug of mucus, which is most commonly, in bronchitis, the source of this affection. This condition is to be found in the form of the tubes.

The bronchi are a series of gradually diminishing cylinders, dividing, for the most part, dichotomously. If a plug of any kind, but especially one closely adapted to the form of the tube, and possessing considerable tenacity, be lodged in any portion of such a cylinder, it will move with much more difficulty towards the smaller end, and in doing so will close up the tapering tube much more tightly against the passage of air, than when moved in the opposite direction into a wider space. If such a plug be placed over a bifurcation, it will, even if freely moving in the larger space in which it lies, be of sufficient bulk to fall back upon one or other of the subdivisions during inspiration, in the manner of a ball-valve upon the orifice of a syringe, and thus completely to occlude it. The consequence of this mechanical arrangement must inevitably be, that at every expiration a portion of air will be expelled, which, in inspiration, is not restored, partly owing to the comparative weakness of the inspiratory force, and in part to the valvular action of the plug. If cough supervene, the plug may be entirely dislodged from its position, or expectorated, the air of course returning freely into the obstructed part; but if the expiratory force is only sufficient slightly to displace the plug, so as to allow of the outward pasage of air, the inspiration will again bring it back to its former position, and the repetition of this process must, after a time, end in perfect collapse of the portion of lung usually fed with air by the obstructed bronchus.

It is not a little surprising that this simple and clear mechanical mode of explaining the collapse should not have occurred to Dr. Fuchs, who, in accounting for the disappearance of the air in the experiments of Mendelsohn and Traube, finds himself reduced to the theory of its absorption into the blood-vessels. Surely nothing can be more superfluous than such an explanation.

Origin of Bronchial Abscess.—The mechanism of this lesion it is not difficult to explain satisfactorily. When pus accumulates in the central bronchi of a collapsed lobule, the evacuation of that pus is prevented from occurring, firstly, in consequence of the absence of the expiratory *vis a tergo;* and secondly, from the resistance opposed by the thickened mucous membrane and its secretion, closing up the bronchus in front. The coats of the ultimate bronchi, therefore, softened and injured by disease, gradually give way to ulceration; and the pus, which thus accumulates in still larger quantity, may at first scarcely be circumscribed, but soon begins to be surrounded by a false memhrane exactly similar to that of an abscess in any other part of the body. The continuity of this membrane with that of the original bronchus, may be either maintained from its first formation, or it may be secondarily established. I believe, however, that the first of these views is the correct one; and that the bronchus acts the part of an obstructed fistulous opening, not sufficiently pervious to prevent accumulation entirely, but not permitting of its increase beyond a certain amount.

When the bronchial abscess has been of some standing, and the patency of the tube leading to it has become re-established in time to prevent its obliteration, a process of repair takes place, analogous to the cicatrization of a wound, and perfectly similar to that which is observed in all healing excavations in the lung, however formed. The false membrane which lines the cavity becomes intimately blended with the bronchial mucous membrane, and indeed comes to resemble it so closely that it is almost impossible to tell where the true mucous membrane ceases and the new structure begins.

This reparation, however, is rarely, if ever, accompanied by restoration of the perfect function and structure of the lung; and on this account it will be considered under the permanent effects of bronchitis.

The length to which these remarks on collapse of the lung have extended, can only be excused by the immense importance of this lesion in relation to the pathology of bronchitis; the whole of the organic affections following from which seem to me more or less dependent on that which has formed the principal subject of these observations.—*Monthly Jour. of Med. Science, Aug. and Sept.,* 1850, *pp.* 122, 230.

36.—*On very Minute Doses of Tartar Emetic in Phthisis and Asthma.* By M. BERNARDEAU.—In vol. xxxi. of the 'Bull. de Thérap.,' M. Bernardeau gave an account of the great benefit he has seen derived from the administration of minute doses of tartar-emetic in the hectic of phthisis. Since that period he has used it in other stages of tuberculization, and in several cases of asthma, with excellent effects. He gives from three to six pills in the twenty-four hours, each containing 1-25th of a grain. By their use, the cough, dyspnœa, and inordinate action of the heart become calmed, and in fact all the good effects of morphia, without its inconveniences, seem to be produced.—*Bulletin de Thérapeutic.—Brit. and For. Med.-Chir. Review, July,* 1850, *p.* 278.

37.—*Pulmonary Diseases in Tropical Climates, and their Treatment.* By R. H. A. HUNTER, Esq., Staff-Surgeon.—[Mr. Hunter glances at these diseases because they have been little noticed in works on tropical diseases, rather than on account of their superior importance. He says:]

Consumption, as we have elsewhere shown (Transactions of the Bombay Medical and Physical Society, No. 4), is a disease of moist exposed localities, without reference either to latitude, temperature, or elevation, and so occurs equally in tropical as in temperate climates. Thus it is found at its minimum in the dry sheltered climate of the East Indies, whilst in the West India islands it attains nearly to its maximum. From inattention to this, very grave mistakes have been committed. Persons have frequently been sent to the West Indies on account of consumption, though this is about as reasonable (according to the above views at least) as to send those with chronic rheumatism or pleurisy to the frozen regions. It is sometimes objected, it is true, that Madeira is a good climate for the consumptive, but Madeira is not within the range of the trade winds, neither is it in "the variables," as they are called, but between these in the mildest region of the world. Madeira, too, is at no great distance (to the eastward) from the dry burning deserts of Africa, whilst from the west it is completely sheltered by the mountains, only the lower south-eastern face of the island being inhabited, by invalids at least; whereas the West India islands are far in the strength of "the trades," after these have swept nearly across the whole western ocean.

[Mr. Hunter does not deem it always prudent to send the consumptive to the southern shores of England, except the summer be decidedly dry; and he says the low inland districts, as Clifton or Bath, might be better, to the sheltered localities of which he would ascribe their salubrity.]

But, he continues, the best place for the consumptive we know, and now within the reach of the wealthy at least, in thirty-five days, is Poona, under the Presidency of Bombay, as it also possesses the advantage of being as healthy as England in most other respects. Madras, too, is a good climate for the consumptive, and Bangalore (another excellent climate); but Bombay, Cannanore, and the Malabar coast generally,

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had better be avoided, at all events in the rainy season. In Bombay, indeed, one way and another, we met with more of consumption than in all the rest of India together twice over. It is there particularly frequent, too, among the "half castes," and those of European extraction generally, who have been brought up in India, insomuch that on our losing one of this class in the female (regimental) hospital, the late Archdeacon Jefferies mentioned he had never known one of the girls brought up in the Bycullah Orphan School reach the age of thirty, and chiefly from this cause. The regiments or detachments from the interior, also, soon lose their consumptive cases in Bombay, whether arriving as invalids or in the ordinary way of relief, and the same at Cannanore, and particularly during the moist stormy winds of the south-west monsoon.

If consumption be confirmed, however, any change only adds to the discomfort, and may hasten the event: nor would it be advisable to go to India otherwise than viâ Egypt, which is itself, I should say, a good climate for the Consumptive about Cairo, not Alexandria, though, it is true, we have the authority of Celsus, that the latter is preferable to Italy.—*Medical Gazette, August* 30, 1850, *p.* 367.

38.—ON DISEASES OF THE LARYNX.

By Dr. John Scott.

(Read before the Edinburgh Medico-Chirurgical Society, 15th June, 1850.)

[An excellent work by Dr. Horace Green, of New York, was published in 1846, in which the author describes, in a plain and lucid manner, the application of the topical remedy for the cauterization of the larynx and pharyngeal cavity in cases of follicular disease. Dr. Scott says:]

Physicians have for many years been in the habit of applying caustic to the throat, tonsils, and uvula, in the solid and liquid form, in cynanche, scarlatina anginosa, tonsillaris, and other acute diseases. The first person, however, to apply the caustic solution to the larynx itself, was our very able countryman, Sir C. Bell. In his 'Surgical Observations,' he relates an interesting case, where the solution was applied in an ulcerated state of the glottis, when the patient was threatened with suffocation, and with a successful result. Another case, similarly treated by Sir Charles, is referred to by Dr. Watson. A full account of these cases will be found in the last volume of 'Forbes' British and Foreign Review;' and also a description of Dr. Vance's practice, in what he calls *swabbing* the glottis, by means of the finger of a glove dipped in a caustic solution.*

In 1836, Trousseau and Belloc described the cauterization of the larynx in laryngeal phthisis, but they seem to have used it with hesitation and reluctance, and not to have appreciated the full advantages to be derived from it, preferring, in most cases, the injection by means of an ingenious syringe. It is to Dr. Green, therefore, that we owe the proper application of this treatment to a common and very troublesome form of disease, which he was the first to describe and properly discriminate. The disease alluded to is denominated by Dr. Green follicular disease of the air-passages, as it consists of a diseased condition of the glandular follicles of the mucous membrane of the throat, larynx, and trachea. The structural changes are—inflammation, which may terminate in ulceration, hypertrophy, induration, or a deposition of tuberculous matter in the follicles themselves. This, from its frequency among the clergy in America, as well as in this country, has been denominated the clergyman's sore throat. It is a form of disease which we have all occasionally met with, and experienced great difficulty in treating. I have always been in the habit of employing the nitrate of silver, either in a solid or liquid state, or the sulphate of copper, but by overlooking the diseased state of the epiglottis and larynx, the cure was rendered more tedious and unsatisfactory, and in many cases the affection was considered as incurable, and mistaken for some more serious disease. Dr. Green's plan of treatment consists in cauterizing freely the whole diseased surfaces by a strong solution of crystallised nitrate of silver, generally from forty to sixty grains to the ounce of distilled water. The tongue is depressed, and the epiglottis brought into view by means of a jointed spatula.

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* See also Retrospect, Vol. xvi., page 124, and Vol. xx., page 88.

The solution is applied by means of a curved rod of whalebone, to which a small piece of sponge is firmly attached. In many cases it is sufficient to cauterize the throat, and to press the sponge against the orifice of the glottis, without attempting to pass it into the wind-pipe. In other cases, it is necessary to pass it fairly into the superior aperture, and down to the chordæ vocales. This is in general accomplished with little difficulty. I am aware that the possibility of doing this has been denied, but I can only say, that I have done it repeatedly, and found it to cause far less irritation than might have been expected. To accomplish this object, the sponge is carried steadily, without touching the pharynx, behind the erect epiglottis, drawn slightly forwards, and pressed steadily downwards, when it readily slips into the glottis, which is known both by the feeling of gasping, and the spasm immediately induced in the patient. The sensation is peculiar, though not painful in general. By causing the patient to inspire fully, the operation may be facilitated. The spasmodic action squeezes the fluid from the sponge, and it is thus freely applied to the diseased surface, causing an immediate and profuse discharge of mucus. It is always advisable, before passing it into the glottis, unless in very urgent cases, to apply the sponge two or three times to the throat and fauces, to educate the parts, as Dr. Green expresses it. Sometimes a difficulty arises from the swelling of the epiglottis or aperture; a few applications will remove this, and then the sponge can be passed without difficulty.

It occasionally happens that the spasm is severe at the moment, but this soon passes off, and I have never seen it followed by any disagreeable symptoms The disease to which this mode of treatment is applicable, is so fully described by Dr. Green, that it is superfluous for me to enter into much detail.

I shall not refer to the application of this mode of treatment to croup, as no case of this kind has fallen under my own observation. A work on this subject has been published by Dr. Green, where the success has been such as to warrant the operation in this very formidable disease. The same remark applies to hooping-cough, in which this treatment has also been said to be successful. The state of the tonsils and uvula ought always to be attended to, and for these, when seriously affected, excision is the only effectual remedy. There are many cases of acute disease to which this treatment may be applied, and, as already mentioned, English physicians have long been in the habit of applying the caustic to the throat in such.

In diphtherite, by the free application of caustic in solution over the whole surface, the inflammatory crust is destroyed, and a more healthy surface remains. The repetition of the application two or three times a day is required. In scarlatina anginosa I have used the sponge with good effect.

The following case furnished by my friend Dr. Brown, may illustrate its application to acute diseases:—

Case I.—J. S. S., æt. 43, strong, active, and healthy, had a severe attack of erysipelatous sore throat, which required vigorous treatment, local and general. He was recovering when I was sent for, the message being that Mr. S. was dying. Meeting fortunately with Dr. Scott, he accompanied me. The patient had all the appearances of imminent death; his face expressive of intense terror and anguish; the extremities cold; the pulse hardly to be counted, from its rapidity and weakness; the breathing all but impossible, apparently from some affection at the top of the wind-pipe; the voice was gone. On looking deeply into the throat, the pharynx and top of larynx were seen of a deep red. The patient being too weak for blood-letting, and too ill for any slower means of relief, Dr. Scott applied the solution of the nitrate of silver, which he happened to have with him. He got the sponge completely into the larynx. Mr. S. almost instantly expressed by signs his relief. In the evening he could speak a little, and was able to lie down in bed, and was, in all respects, better. He was blistered, and had calomel and opium. Next morning he was much better. The sponge was again applied without any difficulty. He recovered rapidly, and has been for more than two years in perfect health, attributing, without any hesitation, the saving of his life to the sponge and the caustic.

The issue of this case was fortunate, yet I fear we cannot be always so certain of success, and must combine the practice with general treatment. Leeches, antimony, calomel and opium, or bleeding are frequently required. In the early stages of *diphtherite,* where the exudation is commencing on the tonsils and posterior fauces, I have seen the solid caustic, or the solution, arrest the disease. In the very fatal epidemic diphtherite, where sudden depression of the vital powers and death occur in a very short period, I have not had an opportunity of trying its effect. The disease, however, for which the treatment is most usually required, is follicular disease of the throat and larynx, which is a very common affection, and so frequent, that it is surprising how little attention it has attracted. It is met with in all classes, more perhaps among public speakers than others, and more frequently in males than in females.

Follicular disease is often accompanied with cough and slight expectoration,—sometimes mucous, sometimes purulent. In some cases phthisis is suspected, and gives rise to great and groundless alarm. In such a state of matters we must be guided by the stethoscope and percussion. When the chest is sound and the larynx diseased, the symptoms are referable to the latter organ, and disappear under proper treatment. The following is one of these supposed cases of phthisis:—

Case III.—Mrs. C., æt. 23, had suffered from influenza and bronchitis, from which she slowly and imperfectly recovered. There was occasionally a slight appearance of blood in the morning, cough, slight emaciation, febrile accession towards night, and great debility. The tongue was furred, the pulse quick, and the appetite defective. The the chest was found healthy on examination, but the throat was swollen, red, tuberculated, and the epiglottis swollen. General treatment was resorted to, and the throat and epiglottis touched every second day. In a fortnight the symptoms were almost gone, and the patient left Edinburgh quite recovered.

I have also met with another form of the affection, of which the following is an example:—

Case IV.—Mrs. S. had been for some years affected with hoarseness, loss of voice, and great difficulty in breathing in damp weather, so much as to prevent her lying down. The cough came on in severe fits. At times, in warm dry weather, she was tolerably well, but liable to relapse in spring and winter In this case there was tubercle of the throat, and slight ulceration of the epiglottis, apparently extending into the larynx. There was some difficulty in managing the passing of the sponge, and the treatment required it to be applied eighteen times. She left for a milder climate.

Dr. Green, in mentioning the causes of follicular disease, considers dyspepsia as a consequence, rather than as a cause. This is contrary to my experience, as in many instances there is dyspepsia and general derangement of the chylopoietic viscera. It may frequently, also be traced to mental anxiety, over-study, or neglect of hygienic remedies. In all such cases the general health must be restored by sarsaparilla, iodine, quinine, and other remedial means,—a change of scene and relaxation from business are often essential. I have generally found it necessary to give a mild mercurial alterative, with some alkaline and saline medicine, for a week or ten days, at the time the local treatment was going on. In other cases, however, the general health is quite unaffected.

Case V.—Mr. C. had held a situation, which required much public speaking. About two years since the voice became hoarse and harsh. There was no cough or difficulty of breathing; a slight expectoration of mucus; pain above the sternum and under the clavicle; general health good. On inspection, small ulcers were discovered in the throat, and lower down, the fauces were denuded of epithelium, white, and covered with a thin muco-purulent matter. The epiglottis was also thickened. On the first application of the caustic —which, however, did not touch the epiglottis—the patient felt relieved. On the second occasion, next day, I passed the sponge fairly into the glottis, without causing much irritation.

Case VI.—One of the most interesting cases of the kind I have seen, occurred quite lately. The patient was brought by my friend, Mr. Carmichael, who stated that, for more than three years, he had been liable to paroxysms of coughing, so severe as occasionally to suspend respiration, the lips becoming blue, and the patient falling down insensible, and not recovering for some time. From the violence of these fits he had been obliged to give up his business as a merchant. Every means his medical advisers could suggest were tried, without effect; and no progress had been made even in alleviation. The throat, on inspection, was found slightly tuberculated in the upper part; lower down it was seen to be in a state of ulceration, while the epiglottis was also found swollen and ulcerated. On the first application the caustic was passed into the larynx, and a fearful paroxysm of croupy breathing came on, followed by profuse expectoration, and subsequent relief. I have now applied the sponge five or six times, a little more cautiously than at first, but still into the larynx. After the third application, he informed me that he could swallow soup, which he had not done for some time. Sickness and vomiting generally followed the taking of food, but he can now, after three weeks' treatment, take any kind of food, and has gained four or five pounds in weight,—the best indication of improved health; and I trust his future progress will be equally satisfactory.

One remarkable effect of the follicular disease is the extreme mental depression which accompanies it, and a great feeling of debility and weakness. As the amendment in the throat progresses, the mind resumes equanimity, and the strength is restored.

I may likewise allude to a very interesting case, in which I had recourse to this practice with great temporary relief.

Case VII.—A late celebrated artist had been, for a long series of years, affected with hoarseness, occasional loss of voice (at the time I first saw him he could not speak,) difficulty of breathing on change of weather, and a feeling on inspiration, as if there was an obstacle preventing the air from passing. There was also at times a difficulty of deglutition. The general health was not good; and he had suffered much from a variety of morbid affections. The throat was red and swollen, the epiglottis thickened and rigid. On applying the caustic solution, which was done cautiously at first, and from the long-standing of the disease, without much hope, considerable relief was obtained. About the fourth application the sponge was passed into the larynx. Violent spasm ensued, with a strong feeling of suffocation. This, however, soon passed off, and was followed by profuse expectoration, and great relief. After this the patient could lie down and breathe without difficulty. The spasmodic effect was so violent that I really had some misgivings about applying the caustic a second time. The patient, however, a man of great resolution, had none. It was applied again and again, and, though the spasmodic action always returned, it gradually became less, the voice improved, and considerable ground had been gained, when unfortunately, during the intensely cold weather of January, the patient was seized with acute sore threat and bronchitis, and all local treatment was suspended. At times there appeared some amendment. When I saw him he could only speak in a whisper. The larynx, however, was not much affected, except on one occasion, when there was so much difficulty of breathing that I contemplated laryngotomy. Next morning, however, this was gone, and the inspiration and the expiration quite free. Mr. Syme saw him along with me, and agreed that the larynx was not the organ affected. The disease was evidently bronchitis, with disease of the lung; and he sunk exhausted from that cause in April.—*Monthly Journal of Med. Science. July* 1850, *p.* 15.

39.—*On an Improved Mode of Applying Nitrate of Silver to the Interior of the Larynx and Trachea.* By Dr. J. Chew Thomas, Baltimore, United States.—Every one who has tried must be aware of the difficulty of introducing nitrate of silver into the trachea or larynx, either in powder or solution, according to the plan proposed by the French physicians.

If a piece of lunar caustic be held with moderate firmness on a small grindstone which is revolving rapidly, a very fine dust from the salt is thrown off in considerable quantities into the air. By holding the open mouth, within a few inches of the stone, and drawing in the breath, any desirable amount of the powder may be inhaled. I am not in the habit of directing more than four or five inhalations at a time. A very simple portable machine for the purpose, contained in a box, may be made by any mechanic of tolerable skill. All that is required is a stone about five or six inches in diameter, half an inch thick, on the same shaft with a small pulley, that is connected by an elastic band with a wheel, six or eight times the size of the pulley. The caustic, which should be free from copper or nitrate of potass, may be held on the surface of the stone by the hand, or a spiral spring adjusted by a screw. I have been much pleased with the effects of this method, in recent catarrh, obstinate chronic cough, and various affections of the larynx; and I have little doubt that it will be found useful in all cases where the use of the nitrate of silver is indicated.—*Lancet, July* 13, 1850, *p.* 53.

40.—CASE OF LARYNGO-TRACHEITIS TERMINATING FAVOURABLY AFTER TRACHEOTOMY.

By Dr. James D. Gillespie.

(Read before the Edinburgh Medico-Chirurgical Society, 19th June, 1850.)

[In this case Mrs. Donald, aged 30, about seven months pregnant on the evening of the 15th of January, 1850, after exposure to cold and wet was seized with symptoms of ordinary catarrh, to which she paid little or no attention.]

On the 20th, pain in the larynx, with dyspnœa, was superadded. The difficulty in breathing gradually became more intense, but as the patient had been occasionally subject to asthmatic paroxysms, the increasing danger of the symptoms was disregarded. On the morning of the 29th, however, her husband became alarmed, and applied to Mr. Hugh Balfour for medical advice.

He found the patient propped up in bed, unable to occupy the recumbent posture, and suffering from repeated paroxysms of severe dyspnœa. Mr. Balfour ordered leeches to be applied to the larynx, and an emetic to be immediately administered. The leeches were not obtained, but the emetic was taken, and caused violent retching, without, however, any abatement of the symptoms.

The same evening I was requested by Mr. Balfour to see the patient, as he thought it probable tracheotomy would be necessary. I accordingly saw her about midnight, when the following was her condition:—The pulse was about 100 in frequency, weak, but jerking, and every two or three minutes a paroxysm of intense dyspnœa, threatening asphyxia, occurred. On examining the fauces, the uvula and soft palate were considerably injected and swollen, and by pressing down the tongue. I was enabled for a moment to catch a glimpse of the epiglottis, projecting straight up, much thickened and enlarged. This examination caused a violent fit of coughing and dyspnœa.

As the dyspnœa was occurring in well-marked paroxysms, differing in intensity, I deemed it advisable to try the effect of antispasmodic remedies, before resorting to the extreme measure of making an artificial opening in the trachea; so a combination of sulphuric ether, with the muriate of morphia, was prescribed to be taken every half hour, till relief was experienced. Directions were given to summon me should the patient become worse.

Before daylight I was roused by an urgent message—"That Mrs. Donald was dying." On hastening to her house, I found her sitting up. tossing about in bed, gasping for breath, the pulse almost imperceptible, the countenance much flushed, the eyes protruded, and a cold profuse perspiration breaking on the forehead. Soon afterwards Mr. Balfour arrived, and we at once decided that tracheotomy was the patient's only chance of safety from impending asphyxia. Owing to the convulsive movements of the muscles attached to the larynx, and the strong contraction of the sternal portion of the mastoid muscle, which rendered the trachea deeper, careful dissection proved almost impossible. The difficulty was also augmented by the shortness and narrowness of the patient's neck, and the less than usual rigidity of the tracheal rings. During the operation, the flushing of the countenance became changed, first into purple, then a bluish lividity; the pulse entirely imperceptible; the respirations very much prolonged, not above four or five in the minute; so it was imperative to open the trachea without delay. This was accordingly done, and an opening made sufficient for the introduction of a middle sized tube. Its insertion caused violent fits of forced expiration; and by partly closing the orifice, a considerable quantity of venous blood, which had entered the trachea, was expelled. Still, however, the breathing was not so free as might have been expected, and the patient having communicated by signs the desire to say something, wrote upon a slate, that she felt there were some "pieces of skin" to come away. The friends were ordered to watch the patient vigilantly, and clear out the tube repeatedly with a feather. About noon a most violent paroxysm of coughing occurred; and her husband at last succeeded in extracting from the inner orifice of the tube a large piece of firm membranous lymph, having a semi-tubular form. Soon afterwards she was seen by me, the breathing being comparatively easy; but the patient repeated her assertion, that there was much more *skin* to come away. Pills, containing two grains of calomel with a little hyosciamus, were ordered to be taken every four hours.

Jan. 31st.—A large-sized tube has been substituted. Several smaller pieces of membrane have been coughed up during the night. The patient now first experiences pain in the chest; and examination shows, over the whole front, the existence of innumerable bronchitic râles. There is much difficulty of expectoration, resulting in the discharge of very thin viscid sputa, which speedily concrete on the sides of the tube. The pulse is 100, excited, but having little strength in it. Sixteen leeches to be applied to the upper part of the chest, the calomel pills continued, and an expectorant mixture, containing ipecacuan wine, and sweet spirits of nitre, occasionally given.

Evening.—The leeches have been applied, and the bleeding orifices encouraged to bleed freely. Pulse 120, weak. Breathing considerably relieved, but large gushes of a thin gummy fluid come from the tube. A blister to be applied over the upper part of the chest.

Feb. 1st—*Morning.*—Easier to-day; the blister having shifted during the night, has greatly extended the amount of vesicated surface; pulse 90, soft; bronchitic râles not so loud or numerous; breathing less hurried. During the night there passed through the tube a dense piece of membranous lymph. Another portion has been flapping against the end of the tube, but is not detached.

Afternoon.—Dr. Graham Weir saw her with me, and during our visit, I succeeded in detaching, with a probe bent into the form of a hook, a piece of greyish buff coloured membrane, about two inches long and an inch broad, not possessing a perfect tubular character.

Feb. 2nd—*Morning.*—Breathing continues easy. No more membrane has come away, and there are now no symptoms of any more being present. Râles in the chest more dry, and as if the ramifications of the bronchial tubes were clogged with more consistent mucus.

[From February 7th the patient progressed favourably. On the 10th, she coughed up from the throat a piece of membrane an inch long. On the 15th, the wound in her trachea was quite healed. There were bronchial râles on both sides of her chest—dry on the right side. Some swelling and tenderness was felt around the thyroid cartilage, which was relieved by sinapisms and leeches, and on the 23rd she was discharged cured. Dr. Gillespie says:]

I have deemed this case worthy of record, as it appears to me to possess some features peculiar to itself, which I can scarcely find recognised in any systematic work on diseases of the trachea and larynx, nor have I been able to find cases precisely similar detailed in any of the medical journals. All the cases recorded by Louis as examples of croup in the adult. may be distinguished from what we are now considering by the primary disease commencing in the fauces and pharynx, and gradually extending downwards till it affected the larynx and trachea,—in fact, closely resembling, if not identical with, the epidemic disease known under the term of diphtherite, which has been so well described by Bretonneau. Moreover, in some of Louis's cases, the laryngitis was a secondary affection, occurring during the progress, or after the termination, of a debilitating primary disease.

Dr. Stokes has clearly pointed out the difference between the so-called primary and secondary croup; and though he does not deny the possibility of the primary affection occurring in the adult, in his work on 'Diseases of the Chest,' he has been unable to furnish any example of the disease.

Dr. Cheyne, who had most advantageous opportunities for observation, makes the following remark:—"In no part of Britain, I imagine, is croup more prevalent than in Leith and its immediate vicinity; yet, in the course of nearly fifty years of extensive practice, my father has not seen one instance of croup occurring after puberty, while he has attended many cases between the tenth and fourteenth year, both in delicate and robust children"

There is only one case which I can find in the journals closely resembling that which I have detailed above. It is narrated in the volume of the 'Lancet' for 1838, and is entitled, 'A case of Primary Laryngo-Tracheitis.'

The patient was a female, in the eighth month of pregnancy; the symptoms of impending suffocation were imminent; the child died in utero; but tracheotomy was not performed, owing to the woman having been reported to have coughed up a piece of false membrane. The autopsy showed the "the epiglottis erect, hard, and wonderfully thickened, representing the fourth of a sphere, instead of a plane." "The trachea was lined with a buff-coloured membrane, nearly as thick as the rings of the trachea, extending to the bifurcation; it formed a perfect cylinder, except at one point posteriorly, and at the lower part of the trachea, where there was a longitudinal deficiency. The cylinder of false membrane was free inferiorly; it was attached above." "The folds of the membrane forming the edges of the glottis were hard and swollen; the ventricles of the larynx were hardly visible, being nearly obliterated by the inflammatory thickening and exudation."

It is a subject for consideration in this instance, whether tracheotomy should not have been resorted to, as a chance of saving the patient's life. When we reflect on the obstructed condition of the glottis in acute laryngeal affections, we must at once see the extreme difficulty, if not utter impossibility, of any considerable amount of membranous exudation being expectorated through the greatly narrowed or closed orifice; and it is surely better to give the patient a chance of relief, by performing tracheotomy, than to leave him to certain and immediate suffocation. The difficulty of ascertaining whether any membrane has been formed cannot be urged as an excuse for the non-performance of tracheotomy; for if, as is now almost universally admitted, we are authorised to open the trachea, in the uncomplicated acute laryngitis, whenever the danger of suffocation becomes imminent, that operation is surely more strongly indicated, when we have reason to suspect that the obstruction of a false membrane in the larynx or trachea is superadded to the constricted state of the glottis.

These remarks will not apply to the treatment of croup in children, for many circumstances tend to render the propriety of operation in their cases much more doubtful. The lungs are so frequently seriously affected in the infantile disease, and the bronchi plugged up with adventitious membrane, that careful examination of all the features in each particular case must be made, ere we can resort to tracheotomy, even as a forlorn hope; for we should always keep in view the fact, of the truth of which every practical man must be well aware, that, when a case terminates unfavourably, after surgical interference, the fatal event is certain to be ascribed to the operation; and thus discredit may be brought on what, when employed with judgment, might prove a valuable acquisition in surgery.

That Mrs. Donald's case, though it has some features in common, cannot be strictly ranked as an illustration of primary croup, is, I imagine, sufficiently evident. I am inclined to consider it as an extension of the common acute inflammation of the larynx to the trachea, where, owing to some idiosyncrasy on the part of the patient, it assumed the form of lymphatic exudation.—*Monthly Journal of Med. Science, July*, 1850, *p.* 25.

41.—CASE OF CHRONIC EMPYEMA TREATED SUCCESSFULLY BY PARACENTESIS.

By Dr. A. Peddie, Physician to the Minto House Hospital and Dispensary, Edinburgh, &c.

[In this case R. Neilson, aged 26, was admitted into the Hospital on the 27th Oct., 1847, recommended by Dr. Alison to Prof. Syme for the operation of paracentesis thoracis. It appeared from the statement of the patient he had laboured under pleurisy, from which he never perfectly recovered. His symptoms on admission, were general debility, breathlessness, and palpitation on the slightest exertion, with cough on attempting to lie on the right side. The external appearances, with the various evidences of percussion and auscultation, left no doubt that the left side of the chest was filled with fluid, and on the right there was considerable dulness on percussion over the whole anterior surface; but particularly great between the sternum and mamma downwards, the heart occupying this position. The respiration, except in this part, being very clear and puerile. It being considered that paracentesis, cautiously performed, held out the only prospect of recovery, Mr. Syme, on the 31st of October, drew off 21℥ of greenish coloured pus, on the 7th of November, 50℥, of a similar fluid were removed, and on the 14th, 34℥, the chest being now apparently emptied. The instrument employed was a trocar about the eighteenth of an inch in diameter, and about two and three quarter inches in length. The parts selected for puncture were different points between three or four inches external to the nipple, in the sixth intercostal space. No general treatment was pursued, except restricted diet, and an occasional purge, with the compound powder of jalap. A large blister, however, was applied on the side, and gr. ij. of the iodide of potass given twice a day. On the 9th of April, he was again tapped, and 40℥ taken away; and on the 15th 26℥ were again removed. The situation formerly punctured was made choice of, and after each operation great relief was obtained. The patient now appeared at different times to have the operation of paracentesis performed. On the 8th of July, 79℥ of pus were removed: on the 30th of September, 112℥; on the 1st of February, 1848, 72℥ were withdrawn; and from this period up to the 4th of January, the operation was performed four different times, draining off on the 18th of May, 1848, 57℥; on the 19th of August, 1848, 37℥; on the 27th of February, 1849, 67℥, and the 4th of January, 1850, 26℥.]

Thus he was operated on twelve different times, and in all 621℥ of pus—equal to about four gallons—were taken from him. On each successive admission there was a manifest improvement of his general health and strength; he could undergo a very considerable amount of fatigue, and on the six last occasions he returned home on the day following the tapping, without the slightest inconvenience or injury. The changes, too, in the configuration of the chest, more especially in the disappearance of the infra-clavicular fulness, were very marked; and when last seen, the intercostal spaces had much the same aspect as those of the opposite side; and there was now some degree of movement of the side, during respiratory efforts, very different from its previously paralysed condition. More healthy respiration, also, was heard anteriorly, as low as the edge of the pectoral muscle, and also along the back near the spine. The heart now lay close to the sternum on the right side, and its impulse and sounds were perfectly normal. In the three last tappings I found that considerable force was necessary to introduce the trocar, owing, apparently, to the thickness and hardness of the pleura. This was particularly the case in the last operation; and, although the canula of the trocar measured two and a half inches in length, it seemed short enough for the depth of parts to penetrate. I may remark, also, that the fluid drawn off, although secreted in greatly increased quantity from November, 1846, to September, 1847—at which time it was going on at the rate of 56℥ per month, from that period until the 4th of January last, it came gradually down in quantity, until it had arrived at a ratio of little more than 2℥ per month. It is interesting, also, to remark, that while the fluid was sero-purulent at first, and afterwards became pure pus, that at the last tapping a considerable amount of serum separated from the pus. This change, viewed in relation to the progressive improvement of Neilson's health in all respects, made me feel confident that a cure would soon be complete; and, accordingly, I am glad to report that, in a statement which I had from him a few days since, he says that he considers himself quite recovered from the affection; that he has felt so strong and well since he was last under treatment, that he recently entered into the bands of wedlock; and is convinced that no farther operative interference will be necessary. He states that he is now capable for any ordinary exertion, can lie on either side without oppression of breathing; and does not consider that there is any material change in the configuration of the chest—unless it be a slight depression of the shoulder, and tendency to stoop, which, however, he states was the case before the inflammation occurred. Of course, when a cure in this case is said to be complete, I admit that there is a lung, of which, perhaps, less than one-third only is fitted to perform any part in the function of respiration; that there is an enormously thickened pleura, with strong adhesions connected therewith; and that there is a dislocation of the heart from its own compartment of the thorax, to which it can never return. Notwithstanding all this, I regard Neilson's case as satisfactorily cured, seeing that the functions of respiration and circulation are now carried on compatibly with existence; that the strength is re-established, and fully adequate for the duties of his calling; and that, instead of feeling life as a burden, he is now in a condition, and is seeking, to enjoy it.

The most promising feature in the case was its being a simple pleural accumulation, uncomplicated with other thoracic disease; and there can be no question, but that had an operation been earlier performed, the affection would have been brought sooner to a satisfactory termination. Respecting those cases of hydrothorax, inflammatory or mechanical, which may, or may not, at an early period, be treated by paracentesis, it is not at present necessary to give an opinion; but it appears plain that, as a general rule, whenever pus is detected in the pleural sac, it ought to be evacuated in this way; and that the operation be repeated from time to time, as the breathing begins to be embarrassed in consequence of a fresh accumulation. Should there be a decided prominence or protrusion at any one point of an intercostal space, and more especially if attended with pain or tenderness and discoloration, indicating that the matter is seeking an outlet for itself, the best practice is to allow it to point well, allowing nature to perform her own work, unless the symptoms of dyspnœa, &c., are urgent, when an artificial opening must be immediately made. Even in such circumstances, however, it is advisable to draw off the fluid by tapping in the first instance, rather than by incision, as it is impossible to predict what the diseased membrane will bear in the way of a wound inflicted on its surface. In cases complicated with pneumothorax the case is different, for much greater freedom may be used; although it is perhaps most judicious to make the incision small at first, and dilate more freely a few days afterwards. I have in this manner treated successfully several interesting cases of this serious affection, in which there was reason to believe that the fistulous communication with the lung had been formed by an empyema bursting through its substance. In Neilson's case it was considered proper at first to withdraw only a portion of the fluid at a time; but it was soon apparent that the whole might, with safety, be evacuated at once; and this practice I should not hesitate to adopt in any future case, with the precautions to be afterwards noticed. In reflecting on the history of the case, the impunity with which the various operations—twelve in all—were practised, and the manifest relief derived from each, it is impossible to avoid the conviction of the unlikelihood, or rather the utter hopelessness, of expecting that a cure could have been accomplished by the long-continued use of blistering, iodine, and mercury, which would have been trusted to by some practitioners. The main objection to paracentesis thoracis has been the apprehension of air finding an entrance to the pleural sac, and thus exciting violent inflammatory action. To avoid this supposed danger in Neilson's case, all due precautions were taken.

These consisted, as I have already noticed, in the employment of a very small sized trocar and canula, so as to make it impossible that any large rush of air inwards could take place, and to insure more certainly the immediate closure of the wound; in prohibiting conversation during the flow of the fluid, so as to maintain equal and easy inspirations, and prevent any suction power being exercised from within; in the change of position during the operation,—turning the body over, cask-like, when the stream was becoming feeble; and in withdrawing the canula the instant the stream became interrupted. I agree with Drs. Townsend and Roe in thinking that this source of danger from paracentesis,—the admission of air during the operation,—has been much exaggerated, which is proved by abundance of surgical experience, both in operations with the trocar, when air was known to have entered freely, and with the lancet and bistoury in cases of pneumothorax, when its exclusion was not sought. The experiments of Nysten and Speiss also sufficiently prove that air introduced into the pleura is invariably removed by absorption in the course of a few days. Dr. Roe, however, says, "It is clear that if we evacuate more fluid than can be replaced by the expanding lung, air *must* fill the vacuum; but as this does not seem to be attended with either danger or inconvenience, I think it better to evacuate the whole of the fluid at once with the trocar." Now, admitting the truth of the well-known saying, that "nature abhors a vacuum," he keeps out of view altogether the extent to which the collapse of the thoracic parietes on all sides by atmospheric pressure will, in such circumstances, assist in diminishing any space which the partial replacement of its contents, besides the expansion of the compressed lung—small though it may be—and the ascent of the diaphragm, cannot accomplish. All this is the more likely to occur, provided paracentesis is performed with the precautions already recommended.

Some surgeons have treated cases of empyema by leaving the canula or a catheter in the wound. Dr. Roe objects to this fatal practice, "becase it allows a *continual* ingress and egress of air" to the pleural sac, and thereby excites inflammatory action. Now, although this source of danger must be very great in any case, uncomplicated with pneumothorax—or in which nature has not herself prepared for a fistulous opening, the main danger would arise from the direct and continued irritation of the instrument left in the wound, acting as a foreign body by pressure on a membrane predisposed to diseased action. It is strange that this latter explanation, rather than the former, did not occur to Dr. Stroud, when remarking on a case published by him, in which a fatal result followed the use of an elastic gum catheter after paracentesis.

It is from the infliction of a similar injury that bad consequences generally follow the old method of performing paracentesis. It consists in making an incision in the intercostal space, drawing aside the skin, and then penetrating the pleura with a common hydrocele trocar. It is a clumsy expedient, possessing an appearance of scientific ingenuity, with the view of valving the internal part of the opening, and obtaining a better closure of the wound. Unfortunately, however, it often happens that the wound either does not heal by the first intention, or soon breaks open again; and when that occurs in a bad subject, or where there was no preparation made, as I have already remarked, for a fistulous opening, mischief is almost inevitable. The danger, however, is not from air admitted during the operation, but partly from its admission subsequently and constantly, and more especially from the violence done to the pleura by the operative procedure itself. This method contrasts ill with the simple and safe practice adopted in Neilson's case, in which, although twelve times tapped, on all these occasions there was an immediate closure of the trifling wound, and in none the slightest approach to inflammatory action.

A remarkable feature in Neilson's case was the extent to which the heart was displaced, for its apex was found to beat fully an inch external and inferior to the right nipple. That this should not have occasioned a greater change in its action and sounds is unexpected, although quite in accordance with the observations of Laënnec and others in cases of less extreme dislocation.

This case also points out in a very clear light the sure marks which characterise a pleural accumulation, and distinguish it from other diseased pectoral states. The displacement of the heart alone, viewed in connection with the general symptoms, was quite sufficient to prove the existence of a large effusion in the left sac; but the nature of the case was made still more evident by the general enlargement of the chest on this side discoverable by the eye; the fulness of the infraclavicular region; the disappearance of the intercostal spaces generally; and their rounded, although not prominent aspect at the lower and lateral part of the chest. Then there were the very significant signs of immobility during respiration, and the total absence of vocal vibration. No disease could present more precise indications than the above; and although the remaining physical signs furnished by percussion and auscultation were the least important, as they always are in this affection, they lent their evidence to confirm satisfactorily the diagnosis.—*Monthly Journal of Med. Science, Aug.* 1850, *p.* 138.

42.—THE STETHOMETER:

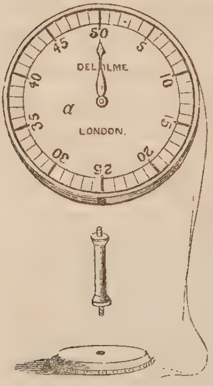
AN INSTRUMENT FOR FACILITATING DIAGNOSIS, BY MEASURING THE DIFFERENCE IN THE MOBILITY OF OPPOSITE SIDES OF THE CHEST.

By Dr. R. Quain.

[In many cases, the modifications produced in the movements of the chest, by disease, are so great as to be at once evident to the eye, or to the eye assisted by the hand; but in the more numerous cases, the eye and hand cannot detect these alterations. As nothing is more common than to find a difference of opinion with regard to the greater or less mobility of a portion of one or other sides of the chest, it is desirable that the mode of investigation should be exact for observation, and definite for description. Although for a long time Dr. Quain was unsuccessful in contriving such an instrument, yet subsequent observations led him to believe that an instrument, simple, portable, and economical, capable of ready application, and of affording very valuable information, might be constructed, different from that of Dr. Sibson, and differing in principle from all others. Dr. Quain says:]

I explained my requirements to Mr. Delolme, chronometer maker, whose ingenuity has supplied many ingenious and beautiful contrivances of an analogous kind. The result has been the little instrument which I am now going to describe, and which has, in the hands of those who have used it, as well as in my own, been found very efficient for the intended object; viz., to measure the comparative mobility of opposite sides of the chest. It is susceptible of many other applications, to some of which I shall again refer, and which will, no doubt, suggest themselves to those into whose hands it may fall.

Description.—The little instrument (see fig.*) consists of a flat case, not unlike a watch-case: on its upper surface is a graduated dial and index, which stands at a maximum number, that may be considered a "zero." This case contains a very simple movement, by means of which the index can be acted on. A silk cord, which may be of a sufficient length to surround one-half, or the whole circumference of the chest, passes through an aperture in one side of the case. This cord acts on the index. When the cord is drawn out, or extended to one quarter of an inch, it will be observed that the point of the index will once traverse the circumference of the graduated dial. In other words, when the index has gone once round, it shows that the cord has been extended one-fourth of an inch. It will be furthur seen, that the dial is graduated, or divided, into fifty equal parts. Each of these parts is, therefore, equal to the fiftieth part of a quarter of an inch; that is, to the 1-200th of an inch.† The index is further capable of going round a second time, on an additional quarter inch of the cord being drawn out. Hence, two revolutions of the index are equal to half an inch of movement—an extent of motion sufficient for all practical purposes. The pedestal and circular foot, shown in the same figure, will be subsequently referred to.



Mode of application.—It is quite evident, that if the instrument be so placed that extension be made on the cord, the amount of the extension will be shown by the movement of the index on the dial. For example, if the instrument is laid flat on the spine, and held in its place by the first and second fingers of the left hand, whilst the cord is carried round the chest, and pressed on one of the ribs, or the sternum, by the fingers of the right hand, then, when the individual under examination expands the chest during inspiration, the amount of expansion will be communicated to the cord, and thus indicated on the dial. The cord may then be directed around the opposite side of the chest, and thus will be at once seen any difference which exists in the relative mobility of the two at the point under examination.

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* The instrument is reduced in this sketch to half size.

† In making and recording observations with this instrument, I am in the habit of expressing a fact thus: right apex, 30; left, 15. It will be understood that the motion is as 15 to 30, without reference to these figures being eighths, fiftieths, or two hundredths.

It will, of course, be absolutely necessary in every examination, such as this, that corresponding parts of the chest be compared. For all useful purposes it may be said, that the movements of the opposite sides of the chest, in health, are identical—the difference which exists over the region of the heart, at the left side, is too immaterial to interfere with practical conclusions. The instrument may be applied to any part of the chest in the mode here described.

[Dr. Quain here introduces a wood-cut representing the position of the instrument when applied on the sternum, and also when applied beneath one of the clavicles. He remarks:]

The latter position is one of considerable importance, from its connexion with the deposition of tubercle towards the summit of the lung. I find that on applying the instrument here the cord may, in this instance, be directed towards the arm more conveniently than in any other direction, and pressed against a point near the insertion of the deltoid muscle. The cord may also be directed downwards, or outwards, and upwards, or inwards, and retained on any fixed point. Nor, as is evident, need this point be a part of the body. It may be on a bed, or on a chair, &c , always providing that the direction of the cord be such as to receive the impression of the movement of the part of the body under examination, and that its position be symmetrical at both sides.

It may be used with the pedestal, which can be fixed in the side of the case with the foot attached as shown in the figure. The instrument thus used is intended to ascertain the modifications of the movements over a limited or circumscribed spot. On this spot the foot is placed and held by the fingers of the left hand, the cord directed towards this point is held between the fingers of the right hand. These fingers, being thus made the fixed point, must be kept steady and not allowed to touch the part under examination. The spring in the instrument is sufficiently strong to resist the gentle tension made by the fingers, and the instrument itself being pushed forward or raised by the elevation of the part during inspiration, the movement of the index, as when the case is applied on the flat surface, becomes the measure of this elevation.

Such being the mode of using the instrument, it will be necessary to say a few words on the precautions which are required in securing accuracy in the result. 1st, It is absolutely imperative that corresponding portions of the chest be examined, and that the mode of applying the instrument, and the point at which it is applied at each side, be identical. 2nd, Care must be taken that the patient breathes in the same manner whilst opposite sides are being examined. 3rd, It should be seen that the cord, when the observation is commenced, is held sufficiently tense to act on the index.

All this requires attention and some little effort; for, as in all other matters, there is little that is of any value which can be obtained without some labour. Though to some few patients these carefully conducted examinations may be irksome, yet to the vast majority they are far otherwise. In the latter, they beget confidence in the medical attendant, and frequently at once inspire a feeling which renders all future intercourse not less pleasing than it is conducive to successful treatment.

Uses of the Instrument. The expansive movements of the chest during inspiration may be said to be the measure of the capacity of the lungs for air. So they are; and by an accurate measurement of the one, we might estimate tolerably correctly the other, and thus be able to compare the relative breathing powers of different individuals. It is exceedingly difficult to make this comparison by external measurement; for the movements of analogous or corresponding parts of the chest, even in those with unimpaired respiratory powers, are remarkably different in different individuals. For not only are such movements modified by age, sex, and occupation, but they are influenced by peculiarities beyond the influences apparently of any fixed or recognized rule. I therefore do not propose to use this instrument in testing whether A has a better breathing power than B. The spirometer of Dr. Hutchinson is a far more correct means of estimating this quality. I propose my instrument as a means of ascertaining any want of symmetry, independently of malformation, and therefore indicative of disease, in the movements of the corresponding parts of the same chest.

It is not my intention to discuss here the various conditions under which this want of symmetry may occur. Obstruction, for example, of a bronchus by a foreign body in, or by a tumour pressing on it, will interfere with the movements of the side of the chest containing the lung to which such bronchus is distributed. Disease of the substance of the lung, tuberculous, malignant, or inflammatory, and emphysema affecting the air-cells, also prevent the free expansion of the lungs or parts of them affected. Diseases of the pleura, viz., acute or chronic pleurisy; bands binding down the lung; pleuritic effusions or tumours in the pleura, have a like effect. Pleurodynia, and diseases of the external walls of the chest, may, of course, interfere with their free movements. However trifling the degree of limitation of motion, which may be produced by any of these diseases, or by many others not here enumerated, I believe this instrument, if properly used, will inevitably indicate it. When such limitation is thus observed, recourse must be had to the study of the general symptoms, and to the other means of physical diagnosis, to discover on what the irregularity depends. Thus, then, attention may be, in the first instance, directed by the use of the instrument to the seat of disease; or, when disease has been suspected or discovered by other means, mensuration will be useful in confirming the diagnosis, and expressing the extent of the local lesion. In both these senses, I have found the instrument an exceedingly valuable aid in the diagnosis of the early stages of phthisis. Indeed, it is quite remarkable how early, and to what an extent, the respiratory movements are modified by tubercular deposit. I hope, however, on some future occasion to return at some length to this and some allied subjects connected with the disease. It will, of course, be evident that if both sides of the chest are equally diseased, we lose the means of comparing them; but such an event is one of very rare occurrence. The instrument can also be used in investigating the movements of diaphragmatic or abdominal respiration, and in taking notice of any irregularity which may be produced in it by disease of any of the organs placed beneath the diaphragm.—*London Journal of Med., Oct.*, 1850, *p.* 924.

DISEASES OF THE ORGANS CF DIGESTION.

43.—ON ASCITES.

By Dr. TODD, F.R.S., &c.

[In a clinical lecture on cases of ascites, at King's College Hospital, Dr. Todd points out that peritoneal dropsy, or ascites, "has its origin in the portal circulation, although not always in the same parts of that circulation," It may also be associated with other forms of dropsy, as when the heart acts imperfectly; or when the kidney is diseased; or when, from the effusion, pressure is made on the ascending cava, thus impeding the return of blood from the lower extremities.]

Of the causes of ascites, by far the most frequent is disease of Glisson's capsule, or of the liver itself. 2ndly. Disease of the peritoneum is not an unfrequent cause, either of chronic peritonitis, or tubercular or cancerous disease of the peritoneum; but these seldom cause extreme distension of the abdomen, unless the morbid mass presses very much on the portal vein or some of its principal tributaries. 3rdly. Tubercular disease of the mesenteric glands, when the disease causes such enlargement of them as to form a tumour, which compresses the mesenteric veins. 4th. Enlargement of the spleen, too, is apt to produce ascites, but seldom, I think, to a great extent, as the spleen has an extraordinary power of adapting itself to changes in its circulation: and likewise because its position does not enable it when enlarged to exercise great pressure on the other tributary veins of the porta. 5th. I have seen, also, a case in which enlarged kidney gave rise to ascites. 6th. Acute peritonitis will produce ascites; this is of a different nature from those previously mentioned, and falls more under the category of acute dropsy. The peritonitis gives rise to an undue accumulation of blood in the capillaries beneath the peritoneum, and the dropsy takes place as a consequence of this congestion or sub-inflammatory condition.

I will now call your attention to one or two conditions which may possibly be mistaken, and, indeed, have been mistaken, for ascites, the necessity of guarding against which fallacy renders the diagnosis rather more complicated.

Sometimes we find in ovarian dropsy that there is a clear tympanitic sound over the tumour; this may be caused by the development of gas in the cyst, either through a process of decomposition of its fluid contents, or by a kind of secretion, as when the bowels generate gas, but more commonly, I think, when a communication is formed between the tumour and the bowel, and the flatus from the latter gets into the ovarian cyst. A short time ago there was an interesting example of this in the hospital. A woman having all the signs of ovarian dropsy was tapped three or four times; there was dulness all over the tumour, and there could be no doubt about the case; when suddenly the tumour became perfectly tympanitic, and, on opening the body after death, a communiaction was found between the ovarian cyst and the intestine, through which the flatus had escaped into it, and given rise to conditions which made the case exactly resemble ascites. If the entrance of flatus had occurred earlier in the case, the diagnosis would have been extremely difficult.

Ascites, with very tympanitic bowels, would present exactly similar signs to those present in this case.

There is another source of fallacy which may be mentioned, as it was one into which John Hunter fell. Sir Everard Home mentions that that great surgeon and physiologist once tapped a *distended bladder*, under the idea that it was a distended peritoneum. We know that patients are very apt to allow their urine to accumulate, especially when they are unable either to feel acutely, or to communicate their wants, as is often the case in fever. The only symptom of the commencement of this distension may be that the patient passes a restless night: the medical attendant, perhaps, does not look for the real cause, and by and by the bladder becomes so distended as to fill the whole abdomen. Now you can easily understand how it would be very difficult to distinguish such a state from ascites. You should always ascertain how the urine is passed; and if this is such as to leave any doubt, you should pass the catheter.

There is only one other thing that occurs to me as possibly mistakeable for ascites, and that is an enormously distended stomach. To what an extent this distension may take place we had an opportunity of seeing not long ago, in a woman who died of chronic gastric disease. The stomach was so much enlarged, that it filled the whole anterior part of the abdomen, all the other hollow viscera being compressed behind and below it. Such a stomach, filled with fluid, may produce fluctuations very analogous to those of ascites. The way to put the matter beyond doubt is to produce vomiting, and cause the ejection of all the contents of the stomach: if, then, the undulating fluid was in the stomach, the fluctuation will immediately disappear. I had an interesting case of this a little time ago in my private practice; and I ascertained the nature of the fluctuation by visiting my patient the next morning before he had taken anything, when the fluctuation had entirely vanished.

Treatment.—You may gather from the details already given in the history of the cases, much respecting the proper course to be pursued. If you can clearly make out that the ascites depends on disease of the liver and of its capsule, and if the liver be not permanently contracted, then the treatment should be directed to the reduction of the chronic inflammation which affects the capsule. With this view the use of mercury should be resorted to, and in many instances it will prove highly useful, and a free discharge of water by the kidneys will often take place simultaneously with the production of ptyalism.

It is also, of course, desirable to increase the action of the kidneys by other means besides. In the cases which I have detailed to you, we used as diuretics, with variable degrees of success, broom, taraxacum, cantharides, nitre, bitartrate of potass, lemon-juice, digitalis, squills; and these are among the best of a very uncertain class of remedies.

I also recommend your attention to the part of the treatment I have made use of with great advantage, viz., *pressure* on the abdomen by means of bandages and strapping. Two of the cases derived great benefit from it, especially the man; he expressed himself (and the feelings of the patient are in such a case very valuable) as much relieved.

A question often arises as to the propriety of *tapping* in ascites. The opinion that I have come to is, that the operation of tapping should be postponed as long as possible; but that that should not be until the distension is extreme. When the distension is allowed to become excessive, the danger from tapping is greatly increased; therefore, if you have tried all reasonable measures and they have failed, do not wait for extreme distension, but tap at once. The danger to be apprehended from the operation is the supervention of peritonitis, and the sinking of the patient. Now the danger of peritonitis may be very much guarded against, by keeping your patient for some time previous to, and during the operation, under the influence of opium.

Still, tapping can be regarded generally only as a palliative measure, and affords very little hope of ultimate cure: nevertheless, it is not, however, perfectly hopeless, for I can tell you of two cases of recovery in my own experience. One of the cases you may have often seen; the woman frequently comes to the hospital for other complaints. She was tapped twice, and I followed the practice I have recommended to you; but with no success. I then tapped her a third time, after which she quickly recovered, and has remained well now for five years. The other case was a man who was tapped for ascites apparently dependent on hepatic disease; he got quite well, and several years afterwards came into the hospital for another disease.

Both of these cases exhibited, to a remarkable extent, a sign to which I have already alluded as not unfrequently present in peritoneal dropsy namely, a dilated and tortuous condition of the epigastric veins. This venous enlargement is probably compensatory, and serves to open up a new and enlarged channel for the blood which cannot be returned through the hepatic circulation. Hence I am disposed to regard great enlargement of these veins as a favourable sign; and I should be more inclined to tap where that enlargement exists than where the veins remained small.

Tapping may be performed with more hope of success when the liver is enlarged than when it is contracted; and it affords least prospect of benefit when the obstructing cause to the venous circulation is extrinsic to the liver, as when it consists of a tumour, or of chronic peritoneal disease. Upon the whole I come to this conclusion that tapping, though a remedy not lightly to be adopted, is not one lightly to be rejected; and though in the majority of cases it does not result in an ultimate cure, yet it is not perfectly hopeless. —*Med. Gazette, Oct.* 11, 1850, *p.* 640.

44.—*On the Treatment of Ascites by Diuretics applied Externally.*—By Dr. Robert Christison, V.P.R.S.E, &c.—[Having read a paper in a French periodical, where digitalis and squill applied in the form of a liniment, had been successful in removing the fluid in ascites, after all other means had failed, and having under his care a boy who had simple but gradually increasing ascites,—but in which the usual means adopted failed in affording relief—Dr. Christison resolved to try the French method of treatment. He says:]

When he came under my care, purgatives and diuretics internally were again tried, and especially digitalis and squill, but with no better success than before. On the contrary, the enlargement and tension of the belly became gradually very great, so that the boy was confined in a great measure to bed. The French suggestion came therefore most opportunely. A mixture of equal parts of tincture of digitalis, tincture of squill, and tincture of soap, was rubbed freely and diligently into the skin of the belly morning and evening. At an early period,—so early, if I do not mistake, as the beginning of the third day,—the urine began for the first time to increase; by and by a copious flow was established; the ascites quickly subsided, and in about fourteen days entirely disappeared; the boy at the same time gained flesh and strength under the use of simple bitters and chalybeates, and ere long his health was completely restored. When I last saw him, four or five years afterwards, he continued well and strong.

The same treatment was tried afterwards in several cases more or less similar to this, but for a time with invariable disappointment. In all, however, the more ordinary measures had previously been exhausted to no purpose; and in most of them decided organic disease of some abdominal organ was ascertained to be the cause of the effusion. At length the following remarkable case revived the hopes excited by the first experiment. A boy, about nine years of age, had been affected with enlargement of the liver and ascites for nearly a twelvemonth. Dr. James Duncan, his medical attendant, had employed all the most serviceable remedies, but to very little purpose. Mercury, iodine, purgatives, diuretics, both vegetable and mineral, had been faithfully given, occasionally with partial effect, but not with any satisfactory amendment. At length the ascitical distension became enormous, the integuments acquired a glistening translucent appearance, the respiration was impeded, and the tightness of the skin prevented the nature and extent of the disease of the liver from being ascertained. But there was no œdema, even in the limbs. In this state I saw him for the first time. Before resorting to the temporary expedient of tapping, it was proposed to make trial of the diuretic liniment; but the integuments were so irritable, that the method by friction could not be persevered with. Dr. Duncan, however, proposed to substitute a strong infusion of digitalis, four times the pharmaceutic strength, and to apply it continuously by linen cloths covered with oil-silk to impede evaporation. In a few days diuresis set in, and a profuse flow of urine quickly removed the whole dropsical effusion. The liver was soon felt to be greatly enlarged, lobulated, and rugose; which disease, it need scarcely be added, proved fatal a few months afterwards. But it is remarkable that the ascites never returned.

I have often used the same method since, both in simple ascites, and likewise where that affection formed a prominent part of a more general dropsy. The issue has of course been variable, and more frequently unfavourable than successful. But on the whole the results of my own observation, and the experience of others in Edinburgh, who have also tried the practice, lead to the conclusion, that digitalis not unfrequently succeeds in this way, when not only it, but likewise all other diuretics, taken internally, as well as the purgative method of cure, prove of no avail. It has likewise been tried with success in a few cases of obstinate excessive œdema of the limbs, in connexion with general anasarca and Bright's disease of the kidneys, after diaphoretics, purgatives, and diuretics internally, had failed to give relief.

The late invention of the texture called Spongio-piline has facilitated the application of this treatment. A large sheet of spongio-piline soaked moderately with a strong infusion of digitalis, made with one ounce of powdered leaves to twenty fluid ounces of boiling water, may be applied to the whole abdomen, or to each limb, and worn constantly without any material inconvenience to the patient. I lately met with a case of Bright's disease, in the University wards of the Royal Infirmary, in which considerable general anasarca, developed to a great extent in the limbs, and concurring, as sometimes happens, with a natural diuresis, was for a time reduced almost entirely by the soaked spongio-piline applied to both legs, although every other customary remedy had been administered unsuccessfully. In another case, that of a dissipated middle-aged woman, who presented all the characters of the variety of Bright's disease which is supposed to depend on chronic inflammation of the kidneys, diuretics given internally removed almost entirely the general anasarca, after diaphoretics had completely failed; the patient thereupon was speedily relieved also of an obstinate chronic bronchitis, liability to vomiting, and tendency to diarrhœa. But the ascitical effusion, which had been predominant from the first, was not reduced at the same time. On the contrary it went on increasing till the integuments became tense. The foxglove infusion was then applied by means of the spongio-piline, with the effect of augmenting the flow of urine in a few days; and then the ascites decreased, at first quickly, afterwards more slowly, till at length all dropsical effusion had disappeared, so that the woman, after seven months of treatment, left the hospital in a fair state of health to outward appearance, and with the urine much less albuminous, and much less loaded with the debris of tube-casts and epithelial cells.

I have tried to ascertain the description of cases in which this treatment is most likely to prove successful, but without any satisfactory result. The pathology of ascites indeed is still not sufficiently understood to allow of an accurate inquiry as to the influence of remedies. Simple ascites, or that along with œdema of the legs and lower half only of the trunk, is known to be a symptom, or rather a sequela, of various organic diseases, such as existing or previous chronic peritonitis, cirrhosis and other organic derangements of the liver, enlargement of the spleen, diseased pancreas, enlarged mesenteric glands, malignant disease of the abdominal lymphatic glands, near the great abdominal vessels, or of any other organ or texture so situated that the tumour thus arising exerts pressure on the great veins. But the physician cannot always distinguish during life, among all these causes, what is the real source of the dropsical effusion; and, on the other hand, several at least of these organic diseases are known to occur not unfrequently without being attended with ascites at all. In these circumstances it is not surprising, that a difficulty is encountered in ascertaining the description of cases in which the ascites is most likely to yield to a particular mode of treatment.

An analogous method, by means of a poultice made with the *Marchantia hemispherica*, or common liverwort, was recommended in 1833 by the late Dr. Thomas Shortt of this city, for the treatment of dropsies in general. I have not found this remedy so generally serviceable as Dr. Shortt seems to have expected, and his method of applying it is irksome to the patient.—*Monthly Journal of Med. Science, October*, 1850, *p.* 310.

45. –CASES TREATED BY KOUSSO IN KING'S COLLEGE HOSPITAL.

By Dr. Budd. (Reported by Mr. Robert C. R. Jordan.)

[Dr. Budd states that the Kousso otherwise called Brayera Anthelmintica, from Dr. Brayer, is a tree growing to the size of the oak, classed amongst the *Rosaceæ*. The flowers, which are medicinal, are used by the natives of Abyssinia as a remedy for tape-worm. Kousso is a brownish powder, (as we see it), very like jalap, and smelling like scammony, with a slightly bitter and somewhat nauseous taste. The price is enormous, 35s. being charged at present for a dose of 4½ drachms. Dr. Budd says:]

Nine doses of the medicine have been given by us, and not one of these has failed to kill and expel the worm.

The medicine has been given in the morning, before breakfast, which is the best time for administering all remedies for tape-worm, as the small intestine which the worm inhabits is then more empty than at other times; the worm, consequently, less likely to be sheathed, and protected from the action of the drug, by the other contents of the bowel

The powder has been infused for ten minutes in three-fourths of a pint of hot water. The infusion has then been stirred, and the whole drunk.

One of the patients, under the care of Dr. Todd, a woman advanced in pregnancy, vomited about half of the dose; but what remained in the stomach destroyed the worm. Two of the other patients had a slight feeling of nausea for ten minutes or a quarter of an hour, after taking the medicine. Another was several times purged by it; another, the woman to whom we first gave it, had headache after taking it, and ascribed to it a diuretic effect. The rest felt no uneasy sensation whatever from the medicine.

In the cases that fell under my own care, I ordered the patients to live sparingly, and to take a seidlitz powder, or a dose of castor oil, the day before taking the kousso, for the sake of emptying the bowels, and so leaving the worm more exposed. This is a precaution which it is well to take before administering any of the remedies for tape-worm. They all act directly on the worm, and to take effect must, of course, be brought in contact with it.

I also ordered the patients to take a seidlitz powder, or a dose of castor oil, after the kousso, to carry the medicine down to the worm and to expel the dead or enfeebled worm from the bowels. This expedient, also, is equally applicable to other remedies for tape-worm, and especially to turpentine, which, if it remains long in the stomach, or passes slowly through the bowels, gets absorbed, and irritates the kidneys and bladder, producing strangury and bloody urine, and may not reach the worm in quantity sufficient to kill it.

Tape-worms are very tenacious of life. They are seldom voided entire without the aid of medicines that act especially upon them. Single joints often come away, and pieces two or three feet long are now and then voided, but it very seldom happens, unless after medicine, that a portion comprising the head is thus passed. This portion remains and grows again, so that a person is often plagued with the parasite for years. One of our patients, Sarah Wheeler, had been so plagued for sixteen years, during which she told us that she had seldom gone a week without passing joints of the worm. She had taken turpentine, and the bark of the pomegranate root, and some years ago, when she was at Fort Beaufort, at the Cape of Good Hope, she took the root of a plant used by the natives as a remedy for tape-worm, which she says is called there "Cacay." She describes this root as being round, and when scraped, white, like a turnip, and sweet to the taste. This medicine had very little effect. The turpentine and the promegranate brought away long pieces of the worm, but the head and the portion near it remained, and the worm grew again. The worm was expelled after the kousso in different portions, on the 11th and 12th of April, and she has since had none of the symptoms, which she attributed to it. She is still (May 31st) in the hospital, for prolapsus of the uterus, which she has long had, but she feels convinced that the worm is entirely destroyed.

Another case, that shows more strikingly still the tenacity of life of the tape-worm, is that of Samuel Payne. He first passed joints of the worm seven years ago. In September last, he was brought into the hospital with cholera, and on the day of his admission passed a portion of the worm two yards in length. He remained in the hospital three weeks, on account of the cholera. Some time after this he again passed joints of the worm, and to get rid of it came to the hospital as an out-patient. Turpentine and castor oil were given him, and brought away a long piece of the worm. The ailments which he attributed to the worm were much relieved for a time. They then became again more severe; joints of the worm were again passed, and in January last he came for the second time to the hospital to be ridded of the worm. Turpentine and castor oil were given him, as before; a long piece of the worm was again expelled, but the creature was not destroyed; so that here the worm had existed seven years, had kept its place during the terrible commotion and flooding of the intestine in cholera, and had escaped destruction by two doses of turpentine. Payne took the kousso on the 3rd of May, and the next morning voided the worm, which was ten yards long.

In all the cases in which the kousso has been given in the hospital, the head of the worm or the taper portion near the head, has been found, so that there is reason to suppose that the creature has been entirely destroyed. The action of the medicine is very speedy. In one of the cases, in which the bowels were slow to move, the worm was expelled, in different portions, in the two days succeeding that in which the kousso was taken; in all the other cases it was expelled the same day, and in several of them after the lapse of only three or four hours.

The result of our experience, then, is, that the kousso is a very effectual remedy for tape-worm: and that in its action it is both speedy and safe.

I have had little experience of the male fern. But the kousso is certainly much more efficacious than the pomegranate; and much more efficacious, as well as less disagreeable, and safer, than turpentine. There is a considerable demand for it at the hospital, which, on account of the cost, we cannot satisfy. I trust, however, now that its efficacy is known in this country, which has commercial relations with every part of the world, that a fresh supply will soon be brought here, and that we shall have it at a reasonable rate.

Persons who have tape-worm generally discover the fact by the passing of joints. The worm seems to grow rapidly; and the tail-joints, as they attain their full development, drop off, and are voided with the other contents of the bowel Probably no person suffers very long from tape-worm, without voiding some of these joints. The existence of the worm cannot be safely inferred, and the remedies for it cannot be given with a fair chance of success, unless joints have been passed. It is a common belief that while joints are dropping the worm is feeble, and that the medicines which kill it act more surely soon after joints have been passed.—*Lancet, June* 29, 1850, *p.* 773.

46.—*The Kousso—Banksia Abyssinica.* By JAMES VAUGHAN, Esq., Assistant Surgeon Bombay Army, Port and Civil Surgeon, Aden, Arabia Felix. —[After noticing the interesting report of the cases treated by kousso by Dr. Budd, Mr. Vaughan says]

In the report above alluded to, I observe that the "kousso" is called the "Brayera Anthelmintica," from Dr. Brayer, who first introduced the use of the plant, or rather made its virtue known in Europe. On referring to Bruce's Travels, Vol. vii., Appendix, a minute and accurate description of the medicine is found, as also an engraving of the plant in the volume of plates. That great and much-calumniated traveller calls it, in testimony of esteem for a friend, "Banksia Abyssinica;" and I submit, with all deference, that as James Bruce was the first traveller who brought the plant into notice, the name given by him ought to be retained.

In the appendix to the second volume of the 'Highlands of Ethiopia,' by the late lamented traveller, Sir W. C. Harris, the following mention is made of the "Cossoo," by Dr. Kirk, who was a member of the embassy:

"Hagenia Abyssinica, ('Cossoo') affords, in a cold infusion of the dried flowers and capsules, the famous drasticum purgans and anthelminticum of the Abyssinians. The tree is one of the most picturesque in appearance."

In Southern Arabia, on the sea coast, and along the shores of the Red Sea, it is well known, and considered a most valuable medicine. In the Northern and Southern Abyssinia it is universally used about once a month, the trees being numerous, and the medicine costing hardly anything. At Hurrur, a province bordering on the kingdom of Shoa, it is well known, and used; though the Somali tribes, residing to the eastward, on the N.E. horn of Africa, are unacquainted with its virtues. Bruce mentions that no Abyssinian will travel without it; and I am informed by my friend Lieutenant Cruttenden, Assistant Political Agent at Aden, that such is actually the case, to his knowledge: the few Abyssinians who visit the port of Aden, invariably bring with them a supply. The dose I understand to be a small handful of the flowers mixed with water, in which sometimes tamarinds are infused, the patient, whilst under its effects, keeping far aloof from every one. Its operation is speedy and most effectual.

In a country like Abyssinia, where almost all, if not the entire population, are more or less affected with tape-worm, the "kousso" is a special blessing of providence, as a medicine within the reach of the poorest shepherd, and infallible in its effects.

Mr. Coffin, an Englishman for many years naturalised in Abyssinia, and now a chief of some importance, visited Aden a few years ago, and whilst staying with my friend above mentioned, administered a dose of the "kousso" with the happiest effect, to an officer of H.M. 17th Regiment. Mr. Coffin never travelled without a supply of the medicine, and even during his stay at Aden, took it regularly himself, and administered their customary doses to his attendants who had accompanied him.

I am not aware if the officers of the surveying vessels of the Indian navy, when employed on the coast of Abyssinia and Nubia, used the remedy or not. I believe they did, as I am aware that many of them suffered greatly from the tape-worm; and one of the party, although eighteen years have elapsed, is not free from the disease even at the present day.—*Lancet, Sept. 7th,* 1850, p. 305.

[Mr. Vaughan says he feels satisfied that he will be able to procure a regular and constant supply of this drug in a very short time, and at a very cheap rate.]

47.—*Kousso. Expulsion of Tænia.* By Dr. INGLIS, Halifax.—[As this is the first time kousso has been tried in the provinces, the following case may not be without interest.]

July 18, 1850.—Elizabeth Smith, aged 56, residing at No. 2, Carrier-street, Halifax, states, that some years previous to 1840, she had suffered from indigestion, as evinced by the usual symptoms; that in the early part of that year, the pain and uneasiness, from being general, became more fixed in the umbilical region, accompanied by a constant aching pain in the small of the back, and a frequent sense of extreme faintness. That in March, 1840, she had taken a largely-diluted dose of Epsom salts, which, operating violently, brought off in the evacuation a portion of tape-worm fifteen yards in length. After this there was a temporary cessation of pain, but, at the expiration of a fortnight, five yards more of the worm, unexpectedly, and without the aid of medicine, were ejected. From that time, till within the last two years, portions of tænia, varying from half a yard to seven yards in length, were expelled, either by the use of remedies or by voluntary effort. That the more acute symptoms have, within the last two years, gradually diminished, but that there has been the almost daily voidance of from six to twenty detached links of tænia, generally in the evacuations, but that not unfrequently portions protruded themselves, even when she had been passive or in a state of rest.

The appetite has been capricious, never ravenous; and the bowels throughout have been regular, although rather inclining towards relaxation.

The usual remedies have been administered, in a greater or less degree, during a period of ten years, and still there continues the daily discharge of portions of the worm, some of which have this day been examined by myself and my friend Mr. Bramley, with whom was last entrusted the treatment of the case.

The several portions of the tænia, when voided, have hitherto been alive.

July 19.—The dose of kousso, having been infused for a quarter of an hour in twelve ounces of hot water, was taken at three separate draughts, an interval of five minutes elapsing between each, at a quarter past eight o'clock, a.m.

The patient having taken the kousso as directed (fasting), a slight degree of nausea was induced, which, however, was soon relieved by a cup of weak tea, without either sugar or milk, and a little toast.

Slight griping pains were felt in the bowels at a quarter past nine o'clock; and at half past nine o'clock there was a free, liquid, and dark evacuation, in which floated many detached portions of tænia. At ten o'clock, another large, dark, liquid motion brought with it the entire remaining portion of the tænia, the head and neck separated from the body, and the body, six yards in length, divided into unequal lengths. Neither portion evinced any symptom of life, as the segments previously voided had done.

A seidlitz draught was ordered in the afternoon, and a lavement of hot water to be used before going to bed.

July 20.—The bowels were frequently moved yesterday after the seidlitz-water, and the lavement was only retained about five minutes. The patient expresses herself this morning as perfectly free from pain; the bowels have been freely evacuated, but not the slightest trace of tænia could be discovered in any of the discharges since the voidance of the entire worm at ten o'clock yesterday morning.

P.S.—The kousso used with so much success was obtained from Messrs. Savory and Moore, of Bond-street.—*Med. Times, July* 27, 1850, *p.* 83.

48.—*On the Treatment and Prevention of Habitual Constipation.* By Dr. J. C. WARREN.—Fine flour is highly nutritious; and if exclusively used, like other nutritious substances, as jelly, arrowroot, milk, &c., causes costiveness. The artificial separation of the covering of the corn is counteractive of the intentions of nature, this stimulating the intestine to expulsive action. After employing, with great advantage, bread containing the *bran* in his own family, and recommending it to numbers of others, the author was induced to try a still coarser preparation. Wheat was ground in a coffee-mill, and then boiled with a succession of water and a little salt for three or four hours. This Dr. Warren has found incalculably *the best and pleasantest remedy for constipation*, effecting quite a revolution in the economy and health, when taken in sufficient quantities (12 oz.), either as a part or whole of the breakfast, or instead of pudding and vegetables at dinner. When the stomach will bear sweet substances, honey, molasses, &c., may be added with advantage. A moderate degree of fluidity, i. e., less than that of boiled rice or hominy, increases the laxative power. The wheat acts in part by its mere bulk, and probably in part by reason of the stimulating effect of the sharp edges of the particles of bran.—*Amer. Journ. Med. Sc.*

[Although the fine bread so much consumed in towns is doubtless frequently a cause of constipation, yet brown bread, containing the bran, cannot be always indiscriminately substituted. Upon this point, M. Bouchardat has recently published ('Journal de Pharm. et de Chemie,' vol. xvii, p. 277, 1850) some interesting observations, in reply to an enquiry by the Paris municipality as to the propriety of substituting fine bread for the coarse brown supplied to the indigent. He observes that it is quite true, as stated by M. Millon, that by rejecting the outer covering of the corn, we reject a substance rich in gluten and fatty matter, and one which, except 10 per cent of ligneous matter, is assimilable. Physiology also teaches us that a residue is requisite for the constitution of the alimentary bolus which is to traverse the canal. A natural condiment is prematurely removed when we reject the bran. But for the system to utilise this portion of the corn, the stomach must be capable of digesting it. An ox or a cow can assimilate it completely: and a country peasant, working freely exposed to air and light, and employed in fatiguing labour, digests his coarse bread easily, and utilises the bran completely. But if you give the same bread to poor and aged persons, the bran will traverse the canal unchanged, the soluble matters it contains being defended from solution by their strong cohesion, and by the covering of ligneous matter. For such persons white bread is alone suitable, and the same observation applies to other persons having weak digestive powers, who indeed, Dr. Warren says, are not able to take the boiled wheat in sufficient quantities. ED. B. & F. M. Rev.] *Brit. and For. Med. Chirurg. Review, Oct.*, 1850, *p.* 538.

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49. *On Alimentation.*—An animated discussion on this important question is now going on at the Institute, between M. de Gasparin and M. Magendie; but science does not seem to have yet revealed to us her "last word;" and, accordingly, the opposing statements and theories of

her servants only render the subject more obscure. M. de Gasparin still insists that the nutritive qualities of nutritious substances may be estimated according to the quantity of nitrogen which they contain. Here practice fully supports the theory. Practical farmers are well aware that ten pounds of hay are equivalent to six and a-half pounds of corn. Now, as the former contains about 1·15 per cent. of nitrogen, and the latter 1·77 per cent., it is evident that experience but confirms the results of analysis.

In the small towns of the south of France the workmen live alternately on meat and vegetables. Long experience has taught them the equivalents of each; and what are they? 250 parts of meat are equivalent to 150 parts of dry haricots. Now the former contains 2·42 per cent. of nitrogen, the latter 3·80 per cent.; and here, again, the theory is consonant to the results of experience. M. Magendie, to all this, replies by an experiment, which he regards as decisive. A carnivorous animal requires exactly the same weight of dried flesh for its support as raw flesh; but, as the food in drying loses nine-tenths of its weight without getting rid of any of its nitrogen, the animal fed on dried flesh consumes nine times more azote than the other.

The experiment, however, cannot be regarded as a fair one, because the dried powder may disturb digestion by the enormous absorption of fluid from the digestive canal. Besides, it is well known that travellers carry with them quantities of dried flesh, which they use by adding to it some water—thus obtaining a great economy of weight under the same amount of nutritious matter.—*Med. Times, July* 6, 1850, *p.* 20.

50.—*On Artificial Feeding.* By Dr. CHURCHILL.—[The following article of diet is much recommended, and is called by the author "bread-jelly."]

"A quantity of the soft part of a loaf is broken up; and boiling water being poured upon it, it is covered and allowed to steep for some time; the water is then strained off completely and fresh water added, and the whole placed on the fire and allowed to boil slowly for some time, until it becomes smooth; the water is then pressed out, and the bread on cooling forms a thick jelly, a portion of which is to be mixed with milk or water and sugar for use as it is wanted. The steeping in hot water, and the subsequent boiling, removes all the noxious matters used in making the bread, and it both agrees very well with the child, and the child likes it very much."

[Dr. Gumprecht, of Hamburg, called the attention of the profession to a novel kind of food, viz., "carrot-pap."]

Wakenroder gives the following analysis of the expressed and inspissated juice of the carrot:

Oil, fatty	1·00	Sugar	93
Oil, ætherial		Malic acid	
Albumen, veg.	4·35	Starch	
Karotin	0·34	Lime	
		Alumina	
		Oxide of iron.	

But as the scraped root of the carrot contains other matters than the above, especially a large quantity of ligneous substance, which cannot be digested, and which will remain often two or three days in the intestinal canal before being discharged, and be highly detrimental to the delicate mucous membrane of children, it becomes important to separate that which will be useful from that which will not. Gumprecht advises an ounce of finely-scraped full-grown carrot to be mixed with two cupfuls of cold soft water, and allowed to stand for twelve hours, being frequently stirred during this period. The fluid portion is then to be strained off, what remains being pressed to yield some more. This fluid is then to be mixed with the proper quantity of biscuit powder, or bruised crust of bread, or arrow-root, &c., and the pap placed over a slow fire, until it begins to bubble. Care must be taken that the heating be not pushed so far as to cause boiling, or the albumen will coagulate. After its removal from the fire, it is to be sweetened with a due amount of white sugar. Dr. Gumprecht states that by mixing the carrot juice with biscuit, crust of bread, or arrow-root and sugar, we obtain all the farinaceous and nutritious elements required, viz., albumen, gluten, starch, sugar, fat, and the phosphates of lime and magnesia. This food is more particularly adapted for children who have been suckled and are being weaned; for those who are being brought up by hand the following preparation is deemed more advisable: an ounce of *very-finely* scraped yellow carrot, and two drachms of biscuit-powder are to be mixed with two cupfuls of cold soft water. This must stand in a covered vessel, in a cool place, for twelve hours, and be frequently stirred during this time. It is then to be drawn off, or strained through a linen cloth. Some sugar-candy and a pinch of salt are to be added to the fluid, which may then be administered by means of the sucking-bottle; care being taken that the food is at the proper temperature.—*Brit. and For. Medico-Chirurg. Review, July,* 1850, *p.* 137.

[The above preparations of the carrot as diet are contra-indicated when there is any tendency to diarrhœa.]

51.—*On the Intestinal Discharges in Malignant Cholera.*—[In a letter to the editor of 'The Lancet,' Mr. GEORGE LOWTHER, of Hull, comments upon the following passage from the concluding lecture of Mr. Simon, on Paludal Poison and Cholera:—"The eliminative acts excited by the poison of cholera, are among the most violent manifestations of disease, stripping off the epithelium of the intestinal canal, and purging forth serum by stool and vomit, with a rapidity which literally desiccates, and shrivels the patient, so that his blood, by reason of its extreme inspissation, is rendered incapable of its normal function." Dr. Lowther remarks:]

During the prevalence of cholera in Hull, I had an opportunity of examining the varied manifestations of this fearful malady, and my observations were not restricted, but, on the contrary, comprised innumerable cases, and if my attention was directed to one set of phenomena more than another, it was to the nature of the intestinal discharges.

Now, if by the term serum, Mr. Simon intends to imply that the discharges differ in no respect from the fluid, strictly so called, which forms so large a component of the blood, my experiments lead me to differ from him entirely.

With scarcely an exception, I submitted the discharges to the test, by a careful examination, and although in every instance free alkali existed, in no case could I detect the presence of the smallest proportion of albumen.

On the contrary, should Mr. Simon intend to designate the intestinal effusions, charged with alkaloid salts only, by the term serum, then the phrase must necessarily tend to create considerable ambiguity.

Were the term serosity substituted for that of serum, in reference to the intestinal fluxes, it might be calculated to convey a more definite meaning;—*Lancet, Sept.* 21, 1850, *p.* 357.

DISEASES OF THE URINARY ORGANS.

52.—ON THE EFFECTS OF EXCRETION IN THE TREATMENT OF DISEASE.

By John Simon, Esq., F.R.S., &c.

Evacuative drugs have held the most important rank in the *Materia-medica:* and there have been times, even in the modern history of practice, when the balance of medical opinion has inclined itself towards the imagination, that by appropriate evacuants any conceivable disease might be expelled from the body, in form of urine, sweat, stool, or vomit. All scientific generalisations, especially such as fall within scope of the popular eye, become liable to these preposterous excesses of application; but it would be a fatal mistake for the interests of medicine, if, on account of such extravagances, we refused to recognise those invaluable indications which may be derived from a careful study, and a rational application of the humoral pathology.

Taking for granted, then, that we ought to follow the suggestions of nature as to the curative tendency of certain excretions, and that we ought, in a large variety of cases, to adapt our treatment to this evacuative purpose, you will perhaps think that the object is an easy one. You will remember the emmenagogues, the diuretics, the sudorifics, the cathartics, the sialagogues, the errhines, the expectorants, of the dispensary; and you will feel assured that, with these resources, you must be omnipotent against humoral diseases,—that, with a pharmacopœia so plentiful, and so nicely arranged, your only difficulty can be that of selection—the merest *embarras de richesses.*

It will surprise you perhaps to be told, unless you have ascertained it in practice before coming to learn it from pathology, how very much delusion lies under cover of those fine names, and how singularly little real or useful power we possess over the organs of excretion.

If we inquire into the physiology of these organs, we find, with them as with all others, that their natural stimulus is the blood; and we are able to say of them generally, that, *cæteris paribus*, their activity of function varies proportionately to the abundance of blood traversing their capillary vessels. To increase the circulation of blood through an excretory organ would appear, then, an easy method of augmenting its characteristic excretions. In the abstract this may be true, but practically it has a very important qualification. The blood is capable of exciting in the several organs of excretion their appropriate acts, only because, by means of its own decomposition, it furnishes to the growing elements of each several organ that specific material which it is their function to appropriate and excrete. If, for instance, the blood were divested of the ingredients of urine, its circulation through the kidney would be fruitless. It is the law of those nucleated cells which form the bulk of all excreting organs, to grow and expand by the appropriation of certain specific materials, and of these only: a cell in the liver fills itself with one stuff; a cell in the kidney with another; and so long as the blood can give them this special pabulum, they grow more quickly and more abundantly, in proportion as their circulation is increased. But, on the other hand, the increased afflux of blood to an excreting organ can serve to stimulate that organ's excretory acts, only so long as the blood is ready to yield to the organ its characteristic materials for excretion. Hence it would appear probable that an excretion can be permanently augmented only by an increased formation of its characteristic materials in the blood; and that the artificial production of hyperæmia in an organ, apart from the condition just specified, could only give a moment's expedition to the process of discharge.

In this argument I assume as an unquestioned fact, that eliminative organs *do not form* the materials which they excrete; that they merely appropriate from the blood certain elements which existed there previously to any act of excretion. The best illustration of this truth is given in the fact, that after absolute removal of both kidneys in the lower animals, urea rapidly accumulates in the blood, so as to become detectible by analysis, and soon in quantities sufficient to destroy life by narcotism; and we are constantly able to observe the same effect in the human subject, where the kidneys are so disorganized as to be incapable of purifying the blood.

It would appear, then, that while the blood, either mediately or immediately, undergoes those important chemical changes which result in its decomposition and decay, the products of this process have their preordained outlets from the body, and so fast as they arise, become evolved; each, as it were, at its own pole of a galvanic battery. I know nothing better to compare it with than the phenomena of galvanic decomposition: you see the blood distributed with uniform qualities throughout the whole area of the circulation, and you see the products of its decomposition appearing with their characteristic signs at the liver, the kidney, the skin; just as, when you plunge the wires of your battery into a trough of water, you get oxygen evolved at one pole, and hydrogen at the other, while the intermediate material remains apparently unchanged. And to apply that analogy somewhat further (though by the way, I must beg you to understand that it is merely chosen for illustration's sake, and that I have no intention of suggesting to you that the vital process in question is of an electrical nature) I would point out this for your notice: as you are quite sure, in decomposing water, that for every volume of hydrogen at one pole, there must be a corresponding half-volume of oxygen at the other,—as you are quite sure that, if the gas be not evolved, it must have spent itself in oxidizing the metal of that opposite wire, so with the manifestations of secretion. You cannot deal with them singly: if the essential ingredients of urine, bile, or sweat, be formed in excess, you are quite sure that certain other ingredients *complementary* to them must have been formed in excess likewise. Supposing for a moment, that the liver and the kidney were the only organs to be considered, it would be a chemical impossibility for the blood to furnish material for one of these glands without likewise evolving, as a necessary residue of that process, the characteristic elements of the other secretion. As an obvious illustration of this, I may cite an interesting observation by Dr. Bence Jones, in respect of the digestive process: when much acid was secreted by the stomach, the urine was found to be alkaline: the excess of acid in the stomach was hydrochloric, and the free alkali in the urine was fixed alkali, and not ammonia: in extreme cases the alkalinity lasted for four hours; as the free acid was absorbed from the stomach, the urine became acid; and this re-action increased, until it affected litmus-paper intensely.

It would appear, then, that the only natural means, for giving increased development to any particular excretory function of the body, would consist in providing for the increased formation of certain specific materials within the blood; and that this increased formation cannot possibly arise as a single local process, but must involve an affection of the entire chemical economy. And it would appear further, that an augmented determination of blood to the excreting organ can only serve to facilitate the process, in proportion as that fluid has previously been charged with the materials to be eliminated.

Now, all the power that we possess of increasing, or appearing to increase, individual excretions, by means of medicine acting through the blood, admits of explanation on the principles which I have stated. We add to the blood the ingredient of some one secretion, or more; and in the increased secretions we find the pharmaceutical material which we have given. Here, however, I ought to state to you that the appropriating power of the various excretive organs is not limited to the exact materials of their normal stimulants. A certain lattitude of operation is allowed, and very few chemical agents (if any) can enter the blood without finding, as it were, some road prepared for their escape from the system —some organ or other ready for their excretion. Thus for instance, iodide of potassium is quite foreign to the animal economy; it has no counterpart in any natural excretion; but so soon as a sufficient quanity has been taken to impregnate the blood, it immediately begins to excite the kidney and to be largely eliminated by it. Accordingly iodide of potassium (as well as various other salts) though perhaps inferior in diuretic force to the natural constituents of the urine, may certainly be considered as acting in the same manner and under the same law.

But how far may this fairly be called increasing the excretion of urine? Suppose, for instance, that half a drachm of nitrate of potass be taken daily by a person in good health; suppose this continued for a fortnight; what will be the total result? More blood will have traversed the kidney; more water will have been secreted; and that waste of water will have been repaired by an increased thirst, calling for drink in proportion; and the increased flow of water will have carried off with it the nitrate of potass, and for the first twenty-four hours will have appeared to increase the urea and the lithates; that is to say, it will have given facilities for their elimination; it will have washed out the tubules of the kidney, and have cleared away all that there was to be cleared; but, except in that almost mechanical process, it will have done nothing for the characteristic excretions of the gland. The urine of the fortnight would be, so far as we know, only the ordinary urine in all respects but one; in addition to its ordinary constituents, it would contain seven drachms of nitrate of potass and a certain additional quantity of water—if, at least, water had been taken in proportion to the thirst. Most of our milder diuretics let their action be resolved into this: that the excretion excited by them consists of the drug itself, *plus* water.

Under the influence of more violent diuretics (such as cantharides or cubebs) given in large doses, so as to cause great irritation of the gland, something different occurs: the excretion is evidently hurried: it contains albumen and tubular epithelium—often blood; it presents at first an increase of lithic acid, apparently at the expense of the urea, and subsequently a decrease of both these ingredients. Finally, so soon as the kidney is relieved from the continuance of these irritating drugs, the specific gravity of the urine (which has already begun to decline in spite of the largest doses) suddenly falls to 1007 or 1008, is quite destitute of lithic acid, and contains exceedingly little urea. These facts (given by Heller as the result of observations carefully made by himself at Vienna) show that the extreme action of the so-called diuretics consists in bringing away the products of urinary excretion in an immature state, mixed with the evidences of inflammation; but they render it little probable, that any purificative action can thus be exerted on the blood; and Krahmer, after performing on himself a hundred and three experiments, of which forty-one were with the so-called diuretics, found that on the whole he passed more urea, more lithic acid, and more of the other solid constituents of urine, without the exhibition of those drugs than with their assistance.

But are there no means, you will ask, of increasing the flow of *real* urine? Can we do no more than add water? Can we invent no real and genuine diuretic, which shall make the urine stronger in its specific ingredients, as well as more plentiful in its flow? Undoubtedly we can, though perhaps in a very different sense to that supposed in asking the question:—*e. g.*, Lehmann, a German analytical chemist, found that by exercise he could increase the proportion of urea in his urine from about 30 in the 1000 to upwards of 45.

I say this is not quite the sort of result wanted, because I presume the giver of diuretics expects that his drugs shall be *specially* and exclusively diuretic; whereas, in the case just quoted, the effects of exercise, no doubt, were to be traced in very many other secretions; in sweat, bile, and so forth. It was, in fact, an illustration of what I have already stated to you, that such secretions are secondary results of a previous chemical change in the blood; the strong exercise and attendant waste of muscle, the increased oxidation of blood, the profuse perspiration, all these were elements in the production of that increased renal secretion. The kidney secretes all urea that is brought to it, and on this occasion, more than usual was brought, because of other chemical changes passing simultaneously in the system. But I can give you another case from the same chemist. He took a scruple of *Thein* (the alkaloid principle of tea) at bed-time; the next morning he found his urine contained about twenty per cent more than its normal quantity of urea. This would, at first sight, appear a case of true diuresis; and as nothing is said of other secretions, I will assume that they were not increased; but if you will call to mind the chemical constitution of the principle referred to, you will see that in the course of oxidation, it might easily reduce itself to the very formula of urea; and I cannot but suspect that something of this sort must have occurred with it, while within the blood; and that thus, reaching the kidney, not as thein, but as urea, it merely appeared in the urine just as if in the latter form it had been artificially injected into the veins. The same effect is produced, and in the same manner, by the injection of lithate of ammonia into the veins, or by its reception in the stomach; for as it enters the circulation, and becomes oxidized, part of it is transformed into urea, part into oxalic acid; both which materials make their appearance in the urine.

I have chosen the kidney for these illustrations, because we have good opportunities of watching its excretory acts; and likewise, because in practice you will hear a great deal about diuretics, and it is as well that you should know how much, or rather, how little, can be done with them. And while I am on this subject, I may show you, as a practical inference, from what I have been stating—a distinction as to the cases in which diuretics can usefully be employed. Suppose that you have a case of ascites dependent on disease of the heart or liver, and you give nitrate of potass, or acetate of potass, or turpentine, as a diuretic, you will have your drug carried off by the kidneys, and with it an increased quantity of water; and if you press your remedy, you will by degrees drain off a considerable quantity of the peritoneal effusion. And in such a case, your diuretic may possibly be a well chosen remedy. But suppose the case to be one of effusion (more generally anasarca) from disease of the kidneys; such anasarca, for instance, as often accompanies Bright's disease; and if it were proposed to give a saline diuretic, you would, I hope, repudiate the proposal. If the blood were examined in such a case, you would find it already containing more than its proportion of the natural diuretic salts; competent knowledge of morbid anatomy would tell you that these are detained in the blood only because of the diminished secreting structure of the kidney; and the addition of a diuretic drug to the blood could do nothing but increase its unnatural state, and perhaps aggravate the kidney-disease by the determination of a larger flow of unavailable blood. It is in such cases as this, that diuretics add materially to the sufferings of the patient, causing albuminuria or hemorrhage.

I believe that what I have stated with regard to the kidney applies equally to all excreting organs: that their best stimulants are their own characteristic excretions; that if these exist in the blood, no extraneous stimulation can be so effective as they, for exciting the organ to which they belong; that if they do not exist in the blood, no special stimulant of the organ which ought to evolve them can do more—even in its highest doses, than bring away from that organ the results of an immature excretory process, admixed with those of inflammatory excitement.

The next question in the pathology of excretion is this: can one excreting organ act vicariously for another? Can the kidneys act for the skin, or the skin for the bowels? or, in short, what does occur when a particular secretion is arrested? A great many cases of the kind are talked of, but, on circumstantial inquiry, they appear very unsatisfactory. As far as I can ascertain the truth, it appears to be thus: when a secretion is suppressed, if there be in the body another organ naturally evolving similar, or partially similar, materials, that organ will, *so far as the agreement of material extends*, but *no further*, adapt itself to the necessity of increased action. And organs which in this manner naturally secrete very similar materials, may be considered (but, again only so far as the similarity extends) to antagonise one another's activity. Thus, in respect of water, the skin naturally antagonises the kidney; if the skin sweat profusely, the urine will be in small quantity; if the urine be excessive (as in diabetes) the skin becomes dry. Accordingly, in the treatment of disease, the skin may be made to transpire a great proportion of water which otherwise would be discharged by the kidney: but the skin would be quite incompetent to relieve the kidney by eliminating urea for it, because that material does not naturally enter into the cutaneous secretion. Probably the only instance in which the relief can be complete is where there are double organs (kidneys, breasts, testicles, for instance) and where, after the loss of one, the surviving other acquires an increase of activity that prevents the system from suffering inconvenience.

In other cases, all the material which cannot escape by the natural excretory surface, or by some other in affinity with it, accumulates in the circulation sometimes to a considerable extent; and then, as all secretions are evolved from the transuded liquor sanguinis, all become more or less contaminated with this retained product, though none are sufficient to discharge it from the system.

But, though the instances of true vicariousness in secretion scarcely extend beyond the discharge of water, yet there are some instances—or, at least, one good one—of compensative secretion approaching very nearly to the vicarious character. You are probably aware that diarrhœa and vomiting are very common incidents in the progress of Bright's disease, when the function of the kidney is much interfered with; and from the recent experiments of two French physiologists, I suppose this symptom must be considered a case in point. Messieurs Bernard and Barreswill extirpated the kidneys of dogs, and watched the result. It consisted of two stages: there was a first stage, marked by increased gastric and intestinal secretion, especially the former; and this new secretion, instead of being periodic, as the ordinary digestive secretion is—instead of having any definite relation to the meals of the animal, went on continuously, just as the secretion of urine would do. During this stage, which lasted above two days, the animal appeared well, digested his food properly, and had no trace of urea in the blood. The second stage was marked by the cessation of this gastric secretion, and by the appearance and accumulation of urea in the blood, which presently produced its characteristic poisonous effects on the brain, and soon killed the animal.

Now; thus far the case looks as if—at least, in the earlier stage of the experiment, the gastro-intestinal mucous membrane had taken on itself the function of the kidney, and had eliminated urea. However, it had not accomplished this vicarious secretion; it had not discharged urea, for the fluid contents of the stomach and intestines were carefully examined, without betraying a trace of that substance; but they contained a very large quantity of ammoniacal salts, naturally foreign to them—so large a quantity, that it was impossible to doubt that the urea had undergone in the blood that transformation into carbonate of ammonia to which it is so prone, and had in this form effected its escape into the intestinal canal. It appeared, however, in all the experiments, that this compensative action could not long go on; the intestinal membrane was apparently unable to keep pace with the necessities of the system, and death soon ensued.

Some pathologists have believed that certain changes in the liver may be considered compensative for previous interference with the lung; as, for instance, where enlargement of the liver or its fatty degeneration occur in connexion with phthisis, or with defective oxygenisation of the blood in hot climates, and under other circumstances of extreme bodily inaction, as with the celebrated Strasburg geese. There is room for doubt in respect of these cases, and especially as to the first of them, for fatty degeneration of the liver often occurs independently of disease in the lung, and, when in connexion with it, is not by any means invariably proportionate to the pulmonary disease. Nor is it, as on this theory it should be, equally a concomitant of other chronic pulmonary diseases; it attaches itself to phthisis by reason apparently of the diathesis in what that complaint arises, not by reason of the interference with respiration, which it eventually produces. Still, I think it may be admitted in general terms, that the elimination of hydro-carbon at the liver will increase when the blood is imperfectly aerated at the lungs, and this may be considered a compensative action, facilitated no doubt (according to a rule I have already given you) by a certain natural similarity in the chemical functions of the two organs.

So far, then, as we have hitherto advanced with the pathology of excretion generally, the following principles would appear established:—

1. One organ can excrete for another only such materials as are common to both. All organs can excrete water, and perhaps certain salts, for one another. Thus far—*i. e.*, in respect of such materials as are common to both—one organ may be said in health to antagonise, or in disease to act vicariously, for another, but no further. There may be a sanguineous derivation from one to another, but not a substitution of activity.

2. To a very limited extent, certain retained excretions may undergo in the blood a chemical change, which brings them within the means of appropriation of some other organ than that to which they specially be-

long; thus in the experiments I cited, urea for a time seemed to be excreted from the system by the mucous membrane of the stomach—not, indeed, as urea, but as carbonate of ammonia; thus, again, carbon and hydrogen, when incapable of passing off in their respective gaseous combinations seem liable to be secreted as fat.

3. By means of drugs having special organic affinities—such as cubebs for the kidney, elaterium for the intestines, arsenic for the stomach—we are able almost indefinitely to produce and accelerate, in the excreting surfaces of the body, certain changes, which tend in each case to eliminate the particular drug with more or less aqueous exudation; but which fail to augment, or, at the utmost, only momentarily augment, the discharge of material specific to the surface. And,

4. As these accelerated molecular changes always involve the occurrence of artificial hyperæmia, so we may expect, and we do really find, that excretions thus abnormally excited will always contain evidence of the congestion we have induced, presenting (just like inflammatory effusions) the several grades of albuminous admixture, to which presently the fibrin of the blood, and subsequently its coloured particles, are added in increasing proportion. Under the influence *(e.g.)* of cholera or elaterium, there is set up along the mucous surface of the intestines, a condition of molecular excitement under the operation of which they first shed their mature epithelium; next, a profusion of unripe epithelial growth, giving the re-actions of albumen, and floated in a profusion of water; next, a fluid which constantly becomes more like the serum of the blood; contains flakes of coagulated fibrin, and often gives evidence of the rupture of capillary blood-vessels.

And as regards the practical application of these powers, it is obvious that by means of them, as by a blister to the skin, we can effect very decided derivations of blood—can render one surface vascular, more or less, at the expense of another; we can likewise carry away, by any of the excretory surfaces, an indefinite quantity of a fluid possessing more or less, according to the degree of stimulation, the chemical characters of serum. But the derivative processes thus induced are so essentially of an inflammatory nature, that we ought very much to hesitate in having recourse to them needlessly or violently; and above all, in respect of solid and complicated organs of excretion, such as the liver and kidney, where every inflammatory excitement leaves its permanent traces of mischief, we ought not to select them for the working of this vicarious irritation without the fullest conviction of necessity.

From the ground we have already gone over, you will have gathered misgivings, that with some show of power against humoral diseases we really possess extremely little true and available influence. For while unquestionably we are enabled to determine blood to this organ, or to that; while we can confidently ensure that our senna shall pass out by one channel, our cantharides by another, and in either case carry serum with them; we find this ability of little service in respect of humoral disease, by reason of what I have already explained to you. If rheumatic fever forms with explosive rapidity certain materials *congenial* to the excretion of skin and kidney, the use of diaphoretics and diuretics is obviously superfluous; and in the latter more complex organ, any such treatment would of necessity do more harm than good. We see the solid materials of the urine largely increased in rheumatic fever: we have the clearest evidence that the material already in the blood is a most efficient diuretic; so efficient, that not infrequently, like cantharides, it produces hyperæmia enough to cause the excretion of albumen or of blood; and we are acquainted with no medicine (unless water be so considered) which can at all facilitate the process thus energetically commenced by nature. Derivatively we may act no doubt on the mucous membrane of the intestines, and may establish there a counter-irritation in relief of the inflamed organs; but against that which is specific in the malady, our purgatives are utterly powerless, and apparently contribute no more to vent its distinguishing "peccant humours," than, in a case of suppression of urine, they would suffice to eliminate urea from the system. Whether a material, seeking to pass off by the skin and kidneys, be normal or abnormal; whether it be urea, or that animal matter which loads the excretions of our rheumatic patients, we have no sufficient reason for believing that we can convert that diuretic material into one voidable by the intestines. By any excreting organ we can only evolve those elements which have a specific and elective affinity for its action; and where this affinity prevails, I repeat that the elements themselves work their own discharge with at least sufficient rapidity. Obviously then, if the science of medicine is to find the means of affecting the course of humoral disorders, we must look further into the operation of drugs than the superficial evidence of their various local affinities. Our only known power of qualifying the specific materials of any excretion lies much deeper in the subject. It lies in such means as we possess for accelerating and retarding the waste of tissues and blood, or that metamorphosis of their material which sooner or later furnishes the elements of discharge. At the head of these means stands bodily exercise, with its attendant increase of oxygenisation, as the natural and by far the most efficient stimulus of the organs of excretion. As to the question, whether there are any drugs which control this process, either to increase or diminish it: here exactly it is that our ignorance displays itself, and that we find our inability to cope with the difficult problems of humoral pathology.

It seems probable that *water* promotes these changes in their normal direction: Becquerel found that, by increasing its use, he could likewise increase the true urinary excretion—that of urea.

There are reasons for believing that mercury occasions in the blood that dissolution of certain materials which is preliminary to their excretion; for first of all (just as with a true humoral disease) there is a period of general uneasiness and febrility; this presently gives way to a second stage of its influence, in which a variety of excretory acts occur with unusual activity; while any effused inflammatory products tend to re-enter the blood, and their fibrin undergoes disintegration. It is not easy to say, whether these phenomena are in the normal direction of chemical change, and whether they affect all the retrogressive elements of the blood; but in one respect the excretions thus evolved obviously differ from the more leisurely productions at the same surfaces—they are more fœtid, and therefore probably less oxidized. Likewise, as with all excited excretions, they are apt to become inflammatory; in mercurial ptyalism the saliva is abundantly albuminous.

Antimony seems likewise, and in the same manner, to accelerate the destructive metamorphoses of the blood; and indeed (since the recent researches of Dr. Mayerhofer) we know more about it than about other drugs of the same class. Without materially altering the proportion of coloured corpuscles in the blood, it produces a marked diminution in its other solid ingredients, and reduces the fibrin to about a third of its usual quantity. Co-incidently with this change occur the various known acts of increased excretion; and in the urine (which has been especially examined) the waste products of the economy are found in excess—especially the urea, of which there is discharged half as much again as is normal.

What other drugs may act in this manner I am unable with certainty to inform you; but when you find any which, like these, tend to affect several excretions simultaneously, you may have reason to suspect that such is their *modus operandi.*

Such drugs, then, as mercury and antimony, when introduced into the circulation, represent exactly the phenomena of true humoral diseases; they effect or hasten a definite metamorphosis in the blood, under the influence of which the materials for excretion become sensibly increased; they do not stimulate the organs on which they act by means of any specific affinity between those organs and themselves (as cubebs stimulated the kidney) but excite their actions indirectly, evolving for their use, from the elements of the blood, a larger proportion of that which it is their normal function to eliminate; and (unlike those evacuative drugs which effect their purpose solely by reason of their local affinities) these *catalytic* medicines, if I may venture to call them so, do not merely add themselves to the excretion which they provoke, as nitrate of potass adds itself to the urine; for no quantity of mercury mixed with saliva would render that fluid fœtid.

Some medicines are said to affect the excretions without having previously entered the circulation. Poiseuille has endeavoured to explain the purgative action of certain saline medicines as a simple phenomenon of exosmosis. He parodied a purge with his endosmometer: he found that serum would pass out of it, through animal membrane, into solutions of sulphate of soda, into seidlitz water, into sea water. The profuse discharge which follows an ounce-dose of sulphate of magnesia consists of nothing more than the ordinary excrements with the addition of the purgative salt and of serum from the blood vessels. Therefore, at first view, there is nothing in the facts inconsistent with Poiseuille's theory; and an Italian physiologist (Dr. Bacchetti) has given further development to the doctrine, by showing that the exosmosis of serum into the saline solution is very much increased when an artificial current is kept up, which (just like the circulation of blood in the living body) constantly brings new portions of the albuminous fluid within the sphere of attraction of the saline one. It is argued further, that some single salt will act as a purgative or as a diuretic, according to its dilution; will purge (by exosmosis of serum) if it be given with little water; will be absorbed and act on the kidneys, if the proportion of water be largely increased.

There is much plausibility in all this, but I doubt if it will bear accurate inquiry. No dilution of sulphate of magnesia prevents it from being a purgative when given in sufficient quantity. No concentration of syrup, or of mucilage, will convert these agents into purgatives, as (on the above theory it apparently should. I have given half an ounce of nitrate of potass twice and three times a day, in a tumbler of water, without producing that purgative effect, which (in respect of its density, and by comparison with sulphate of magnesia) should be expected from its exosmotic influence. The cathartic mineral waters are of very much lower specific gravity than the serum of the blood; of lower specific gravity even than the serum would be in respect of its saline contents alone. Half an ounce of sulphate of magnesia, taken in solution, and acting as a purge, brings away with it far more than enough water to reduce it below the specific gravity of the serum. It seems almost certain, by reason of these objections, that the exosmotic theory of the action of purgatives is an insufficient one. Probably they all enter the circulation; and the difference of action resolves into this; that the kidney suffices, without the aid of another organ, to discharge any small quantity of them which may be in the blood; but if their presence be more copious, the intestinal canal co-operates for their discharge. To this it must be added, that many saline medicines, like other drugs, have definite local affinities, and maintain their adhesion to one eliminative organ, without any regard to differences of dose or dilution; nitrate and iodide of potassium (short of poisonous doses) are not convertible into purgatives; nor am I aware that any dilution of sulphate of magnesia will render it a diuretic.—*Lancet, Aug.* 17, 1850, *p.* 150.

53.—ON THE PROXIMATE CAUSE OF ALBUMINOUS URINE AND DROPSY, AND ON THE PATHOLOGY OF THE RENAL BLOOD VESSELS IN BRIGHT'S DISEASE.

By Dr. George Johnson, Assistant Physician to King's College Hospital.

[Dr. Johnson has already stated, in his communications on the subject of renal pathology, his views on the morbid changes occurring in the secreting cells of the kidney, and his opinion is now confirmed, that the various forms of disease generally included under the name of Bright's disease, and which are, in most instances, associated with an albuminous state of the urine, have this common feature, that "the first morbid change which can be detected in the kidney, consists in an altered condition of the secreting cells." Although we do not possess a complete and accurate history of the secreting cells of the kidney, yet before entering upon the immediate subject of his present communication, Dr. Johnson mentions briefly certain facts connected with them. He says:]

1st. The true secreting cells occupy the inner surface of the convoluted tubes, those portions of the tubes which form the great mass of the cortical substance of the kidney. Neither the Malpighian bodies nor the straight tubes which form the medullary cones contain the true glandular or secreting epithelium.

2nd. No renal cells can be detected in normal and healthy urine. The presence of renal epithelium in the urine affords unequivocal evidence that the secreted products are abnormal. Thus, for example, when during an attack of jaundice the kidneys eliminate bile, the urine contains renal cells deeply tinged with biliary matter. The bile conveyed by the blood to the kidneys has so modified the secreting cells, that they are thrown off entire, and thus appear in the secretion. Various other matters produce a similar modified action of the secreting cells of the kidney, —the fever poisons, particularly that of scarlatina, of measles, and of erysipelas,—certain morbid matters which infect the blood of persons who have had repeated attacks of gout,—others which exist in the blood of cholera patients,—and, again, others which result from an insufficient supply of nutritious food. In these and in some other cases, which do not require special mention, although the poisons are recognised only by their effects, while their essential nature is unknown, there is yet abundant evidence to justify a belief in their existence.

3rd. But it is not only by a process of desquamation that the kidney-cells afford evidence of their having eliminated abnormal products. The cells undergo various other modifications which can be detected by a careful microscopical examination of the kidneys after death. They are sometimes gorged with oil, and this is a condition which may readily be detected by an examination of the urine during life, when, as frequently happens, the desquamative process accompanies it. In other cases the cells are remarkably opaque, and appear to contain an unusual quantity of a finely granular material. I have observed this condition in the kidneys of diabetic subjects, in which cases, probably, the long continued secretion of sugar has modified the action and appearance of the cells. It is of the utmost importance to bear in mind, that there is a most intimate relationship between a secreting-cell and its secreted product, —there is reason to believe, that a change in one will be invariably accompanied by a corresponding modification of the other.

The various changes in the secreting-cells, just now enumerated, produce one common result; viz. an imperfect elimination of the renal secretion. The acute desquamative process rapidly fills the tubes with epithelial cells. The chronic desquamation destroys the life of the cells, arrests their reproduction, and leaves the tubes denuded. The fatty engorgement of the cells tends to obstruct the tubes, and so to impede secretion; and probably each visible deviation from the normal appearance of the epithelial cells is attended by a corresponding imperfection in the performance of their function.

There are, besides, certain other morbid conditions, such as the replacing of the normal epithelium by a deposit of pus or unorganized fibrin in the tubes, which tend to the same result as the more common changes before mentioned. In every instance, excretion is imperfectly performed, and the urinary constituents are partially retained in the blood.

From these preliminary remarks, I pass on to the main subject of this communication, which is to offer an explanation of two of the most common and important symptoms of renal disease, viz., albuminous urine and dropsy; and this will lead me to describe a remarkable pathological condition of the blood-vessels of the kidney, which, so far as I know, has not hitherto been noticed. The observations which I have to offer on this subject will, perhaps, be rendered more intelligible by a previous brief reference to one of those lucid and valuable essays bequeathed to us by the late lamented Dr. John Reid. The essay to which I allude is that 'On the Order of Succession, in which the Vital Actions are arrested in Asphyxia.' The principal facts and arguments therein contained, and which concern us in our present inquiry, are the following:—when the trachea of an animal has been obstructed, by the insertion of a tube with a closed stop-cock, dark blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the animal becomes unconscious of all external impressions. For about two minutes after the animal has become insensible, and when the blood in an exposed and unobstructed artery is equally dark as that in the accompanying vein, the large arteries become more distended and tense than before the stop-cock in the trachea was shut, and when the animal was breathing atmospheric air freely. At the same time, a hæmadynamometer being placed in the artery of one limb, and a similar instrument in the corresponding vein of the other, the former indicates an increase, and the latter a diminution of pressure, as compared with that observed in the same vessels before the air was excluded from the lungs, this evidently resulting from an impediment to the passage of the venous blood through the systemic capillaries. At the expiration of the time before mentioned, viz., about two minutes, the instrument in the artery indicates a diminution of pressure; the mercury, at first falling very gradually, and at last very rapidly, in consequence of the blood being arrested in the pulmonary capillaries, ceasing to pass through the lungs, and so stagnating in the right side of the heart and in the veins.

Dr. Ried then quotes some observations by Dr. Alison tending to show that the arrest of the blood in the pulmonary capillaries "is to be referred to an interesting law in physiology, by which the movement of nutritious juices is influenced by the chemical changes; or, as Dr. Alison terms them, the vital attractions connected with the chemical changes constantly going on in the capillary vessels between these juices and the surrounding tissues, by which nutrition and secretion are effected. Before arterial blood can be transmitted freely through any tissue or organ, it is not only necessary that the contractions of the heart be performed with a certain amount of force, but that the actions of nutrition and secretion be also in operation; so, in the same manner, before the blood can be transmitted through the lungs, it is not only necessary that the right side of the heart retain its contractility, but that the chemical changes between the blood and the atmospheric air should proceed." This doctrine is still further illustrated by Dr. Reid's experiment, before described, which demonstrates that when the blood in the systemic circulation becomes decidedly venous, it passes less freely through the capillaries into the veins. Finally, Dr. Reid refers to some observations by Mr. Wharton Jones, which seem to accord with the explanation here given of the arrest of the pulmonary circulation during asphyxia. Mr. Jones observed, that when a solution of common salt, or a stream of carbonic acid gas, was applied to the lung of a living frog, the circulation of the blood was there arrested by the red corpuscles agglomerating together, and applying themselves here and there flat against the wall of the vessel. The same phenomena were observed, when a solution of common salt was applied to the web of a frog's foot.

The observations and inferences of Drs. Reid and Alison seem to be applicable, by way of analogy, to the subject of renal disease, and will, I think, assist us in our attempt to arrive at the immediate cause of the albuminous condition of the urine and the general dropsy, which are so commonly associated with disease of the kidney.

In tracing the progress of a case of acute dropsy occurring as a consequence of scarlatina, it will commonly, but not invariably, be found that the patient has been exposed to cold. The natural process of elimination by the skin has thus been checked, and the poison is driven inwards to the kidney. It reaches the inter-tubular capillary plexus, and an effort is made to eliminate it by that modified action of the secreting cells which we have called desquamation. The cells are formed and shed rapidly and in large numbers; so that in the course of a few hours, many of the tubes may be completely filled by their accumulated solid contents. This condition of the tubes must obviously impede the secretory process; the blood is imperfectly purified, and excrementitious matters accumulate in it.

We have now to consider the influence which the materials, thus retained in the blood, exert upon the circulation through the kidneys.

Assuming that the renal circulation is affected by an imperfect elimination of the urinary constituents in a manner analogous to that in which the pulmonary circulation is influenced by the retention of carbonic acid in the blood, we should expect to find, that the circulation would first be retarded in the inter-tubular capillary vessels, the obstruction, which will be in proportion to the extent of morbid change in the contiguous tubes and cells, will, of course, exert an influence extending backwards in the order of the circulation; so that the Malpighian capillaries and the arteries which supply them will become gorged with blood. This engorgement is exactly analogous to that of the right side of the heart and venous system observed in animals after death from asphyxia.

There are certain facts which afford a remarkable confirmation of that which, as just now propounded, might seem to be a mere hypothesis, or at best only a probable analogy.

That the circulation through the inner-tubular capillaries is retarded, and that the Malpighian capillaries are consequently subjected to a greatly increased pressure and distension, seems to be indicated by the escape of serum and blood which so constantly occurs during an attack of acute desquamative nephritis following scarlet fever. The serum flows into the tubes, mingles with the urine, and renders it highly albuminous; while the colouring matter and fibrin coagulate in the tubes, and afterwards escape in the form of cylindrical moulds, in which epithelial cells are commonly entangled. There seems no reason to doubt, that the blood, in these cases escapes from the Malpighian capillaries, which lie within the dilated extremities of the tubes. The result is precisely similar to that obtained artificially by Dr. George Robinson, who observed, that when a ligature was placed upon the renal vein of a rabbit, the urine became albuminous and bloody. Dr. Robinson performed this experiment several times, and with an almost uniform result.

But still more satisfactory and conclusive evidence of impeded circulation, and of the precise point at which the impediment occurs, is afforded by the condition of the renal blood-vessels in cases of chronic disease of the kidney.

I have observed in all cases of chronic renal disease, which I have examined since my attention has been directed to the condition of the blood-vessels, that the coats of the arteries and of the Malpighian capillaries are remarkably hypertrophied, while the coats of the inter-tubular capillaries and of the emulgent vein present no appearances of hypertrophy or thickening. I have examined the vessels in different cases, both injected and uninjected, and I have compared them with corresponding vessels in the healthy kidney, and the result has been uniformly such as I have stated. I will now detail the appearances which I have observed in each of these sets of vessels, viz., the arteries, the Malpighian capillaries, the inter-tubular capillaries, and the veins.

Arteries —I have observed, in accordance with the description which Henle has given of the arterial tunics, that the minute branches of the renal arteries have two fibrous coats, the inner being longitudinal, and the outer circular; in the healthy vessel the inner coat is thinner than the outer, but in the diseased condition, I have generally found them of nearly equal thickness. The two coats appear to be of the same nature, and in all probability they are muscular. The thickening appears to be proportionally greater in the smallest arteries, *e. g.*, the afferent vessels of the Malpighian bodies, and gradually diminishes in passing towards the arterial trunks. I have frequently observed the coats, at least three or four times exceeding the normal thickness. The canal of the artery is of the normal size; and pervious until the last stage of this interesting series of changes; when, the secreting cells in the tubes being destroyed, and their vital attraction upon the blood ceasing, the circulation is arrested, and oil globules collect here and there in the canal of the arteries.

Malpighian Capillaries.—What has been said of the arteries applies, with certain modifications, to the Malpighian capillaries. The capillaries do not, in the normal state, present a fibrous structure, nor is there any appearance of such tissue in the diseased vessels. The coats of these vessels are greatly thickened, but homogeneous in structure, and the canal is apparently normal, or perhaps slightly narrowed. The injection sometimes fills the Malpighian vessels very completely, in other instances the tuft is only partially filled; and again, in other cases, the injection proceeds no farther than the termination of the afferent artery. The entire Malpighian body is not sensibly enlarged, but the increased thickness of the capillary walls produces a close packing and crowding of the vessels, so that their outline can scarcely be distinguished. The surface of the vessels is usually smooth, and free from deposit, and the entire Malpighian body presents a peculiar whitish opaque appearance. A hasty examination might produce an impression, that the vessels are bloodless; and in the extreme stage they actually become so, presenting oil globules in or upon them, as already described in the arteries, the entire tuft being atrophied, and the capsule shrivelled: but in the majority of instances, the addition of acetic acid, by rendering the vessels transparent, will show the blood corpuscles within them: thus proving, that the blood had been concealed by the opaque and thickened capillary wall, and that the Malpighian vessels must not be hastily pronounced impervious or bloodless.

Inter-tubular Capillaries and Veins.—The coats of these vessels present no appearance of thickening or of any other morbid change; they seem to be less numerous than in the healthy kidney, and they probably become atrophied and obliterated, in consequence of the arrested action of the secreting cells.

The pathological explanation of these changes in the renal blood-vessels appear to be this. There is an imperfect elimination of the urinary constituents, in consequence of changes in the secreting cells, produced by an effort which they have made to excrete abnormal products. Deficient excretion at once leads to impeded circulation—the obstruction occurring at the very point where the excretion should be effected, viz. in the inter-tubular capillaries. The impediment reacts backwards upon the Malpighian capillaries, which in a sudden acute attack become ruptured, and allow the blood to escape into the urinary tubes; but when the disease has been of longer duration, they become thickened, and permit only the serum of the blood to escape. The thickening of the Malpighian capillaries is probably preservative, and is intended to enable them to bear the increased pressure to which they are subjected during the continuance of the disease.

The thickening of the arteries, which proceeds simultaneously with that of the Malpighian capillaries, affords support to the opinion entertained by some physiologists, that the smaller arteries exert a propulsive influence upon the blood. The remarkable hypertrophy of the muscular coats of these vessels seems to have for its object to assist in driving the blood onwards through the inter-tubular vessels in which the impediment exists. Finally, the gland-cells being destroyed, and the process of secretion arrested, the circulation ceases, the tubular tissue wastes, and oil-globules collect in the canals of the blood-vessels. This appears to be the order in which the various changes occur, and the probable relation which they bear to each other. So far as my observation has extended, the thickening of the vessels appears to be associated with all the forms of deposit in the tubes, and is in proportion to the degree in which the tubes and secreting cells are destroyed, as well as to the duration of the renal disease.

In recent acute cases, the appearances just now described are of course not observed. Hypertrophy of the left ventricle is undoubtedly a common result of disease in the aortic valves; but we do not expect to find hypertrophy after a recent attack of acute disease producing a deposit on the valves.

The subject of renal dropsy appears now to demand a brief notice. Analogy would indicate the very great probability, that this form of dropsy is produced by an impeded circulation through the systemic capillaries, consequent upon the retention of the urinary constituents in the blood, and that the obstruction thus originating is similar to that which Dr. Reid detected by the hæmadynamometer when black blood was circulating through the arteries of the animals which were the subjects of his experiments. There is one fact which, *per se*, is almost sufficient proof, that the systemic capillary circulation is actually impeded, in the way supposed, as a consequence of imperfect elimination of the urinary constituents. I allude to the frequent occurrence of hypertrophy of the left ventricle of the heart in cases of chronic renal dropsy, when there exists no obvious disease of the valves or vessels to account for such hypertrophy. The very frequent concurrence of cardiac and renal disease was long since pointed out by Dr. Bright. In passing under review the chief morbid appearances observed in one hundred cases of renal disease connected with albuminous urine, Dr. Bright thus alludes to the subject of cardiac disease. "The deviations from health in the heart are well worthy of observation; they have been so frequent as to show a most important and intimate connection with the disease of which we are treating; while, at the same time, there have been twenty-seven cases in which no disease could be detected, and six others which, from not having been noted, lead to the belief that no important deviation from the normal state existed. The obvious structural changes in the heart have consisted chiefly of hypertrophy, with or without valvular disease, and what is most striking, out of 52 cases of hypertrophy no valvular disease whatsoever could be detected in 34; but in eleven of these 34, more or less disease existed in the coats of the aorta; still, however, leaving twenty-three without any probable organic cause for the marked hypertrophy generally affecting the left ventricle. This naturally leads us to look for some less local cause, for the unusual efforts to which the heart has been impelled; and the two most ready solutions appear to be, either that the altered quality of the blood affords irregular and unwonted stimulus to the organ immediately; or, that it so affects the minute and capillary circulation, as to render greater action necessary to force the blood through the distant subdivisions of the vascular system.

The latter of the two explanations thus suggested by Dr. Bright, is the one to which analogy would point as the true one. The existence of capillary obstruction being admitted, dropsical effusion appears to be a natural and necessary consequence. It follows, too, that an albuminous condition of the urine, and dropsical effusion into the areolar tissue and serous cavities, must result from precisely analogous conditions, viz., an arrest of poisoned blood in capillary vessels. The hypertrophy of the renal arterial tunics, and that of the left ventricle of the heart, must also be considered as analogous means of overcoming an obstacle to the passage of the blood. It will be evident, that the dropsies here referred to are those which result from an abnormal condition of the blood; renal disease being the most frequent, but by no means the only cause of such abnormal condition. The dropsies produced by disease of the heart, or lungs, or liver, or by pressure on a venous trunk, differ in some respects from those before alluded to; they are all, however, alike in this one important particular—that *an impeded circulation precedes and accompanies the dropsical effusion.*—*Medico-Chirurg. Transactions*, Vol. XXXIII, *p.* 107.

54.—ON ANIMAL CHEMISTRY IN ITS APPLICATION TO STOMACH AND RENAL DISEASES.

By Dr. H. BENCE JONES, F.R.S., &c.

[The subject with which Dr. Jones commences his series of lectures is]

Food.—All substances, vegetable and animal, which are fitted to sustain animal life, consist of ingredients belonging to the four following divisions: 1st, water; 2nd, salts, or ashes; 3rd, organic substances containing very little or no nitrogen,—as fat, vegetable acids, starch, sugar, &c; 4th, organic substances rich in nitrogen,—as fibrin, albumen, vegetable albumen, &c. No two different articles of food probably contain these ingredients in the same proportions; and, accordingly, they vary in their nutritive qualities, those being most nutritious which have the largest supply of nitrogenous compounds. But the absence of any of them is destructive of the perfect fitness of the substance as an article of diet, when taken by itself. "On olive-oil, linseed-oil, or cod-liver oil, an animal may grow fat, but he cannot work on such food alone. For the work of the muscles, it is requisite that the food should be albuminous, not fatty. A horse, for example, for hard work, must have corn, and not hay. To do work, food rich in nitrogen is necessary; to grow fat, food rich in carbon is required." But neither will nitrogenous substances alone suffice for food. Animals fed on white of egg died of starvation. There must be a due admixture of all. This is shown in the composition of milk, the type of the food for infants; and in that of wheat, which may equally be regarded as the type of the food for man, with this exception, that water is requisite in addition to the latter.

One great object of vegetables is to prepare the food for animals. "Vegetables, from the air, the water, and the soil, make compounds which animals require,—substances, that is, which, with the least possible change, can be formed into parts of the animal. Vegetables, from carbonic acid, ammonia, water and salts, form albuminous substances, fatty matters, starch, sugar, whereby the various organs of the body are nourished and enabled to perform their actions. The power of forming higher compounds out of more simple ones is, in vegetables, unlimited. Their power of thus compounding simple substances into more complex ones, is most extensive; whilst, in animals, the power of conversion is probably limited to a simplifying change,—the power of forming higher organic compounds out of lower ones is at present unproved. But, you might say, cannot animals make nervous substance,—the noblest substance in creation, whose action so utterly forbids our comprehension? If we examine this nervous substance, we find it also consists of the four classes, viz., albuminous substance, fatty substance, ash, and water; and the highly-organized muscle contains the same substances in different proportions. The power of animals is shown in their forming the substances which they obtain from vegetables into complex structures and organs; whilst the power of vegetables is shown in the production of new compounds, new arrangements of the elements into higher and more complex bodies,—as vegetable acids, alkaloids, neutral substances, albuminous and fatty matters. The power of adapting forms, and making organs, the formative and organizing power, is more seen in animals than in vegetables. The power of building up new compounds, new substances—the compounding, or substance-making power, is, with some exceptions, perhaps limited to vegetables."

Uric or Lithic Acid.—[According to Dr. Jones' observations,]

The uric acid exists in combination with ammonia, the urate of ammonia being modified in form and in solubility by the presence of common salt, and other saline substances. He has not been able to prove the existence of two compounds of uric acid and ammonia, and suspects that the super-salt, described by Dr. Bensch, was a mixture of uric acid and urate of ammonia. That the acidity of the urine is not caused by such a super-salt, as Dr. Prout supposed, is evident from the fact, that the variations of the acidity of the urine and of the uric acid, instead of coinciding, have an inverse relation to each other: when the urine is very acid, there is usually but little urate of ammonia present; when there is much urate of ammonia, not unfrequently the urine is nearly alkaline.

The quantity of urate of ammonia varies remarkably during the day. A few hours after any food, the amount is increased; long after all food, it is excessively diminished. It is but little affected by differences in the kind of food. A slight excess or increase in the acidity of the urine will cause the precipitation of urate of ammonia. If it be highly acid, a precipitate may occur, even when no excess of the salt exists. If the urine tend to alkalescence, an excess of urate of ammonia will be dissolved, and will show no appearance of a superabundance of urates. The precipitation of uric acid crystals depends only on the acidity of the urine, and is quite independent of the quantity of the urate of ammonia excreted. They constitute by far the most delicate and trustworthy test of an over-acid state of the urine.

In regard to the treatment of these two kinds of deposit, Dr. Jones writes, "If uric acid crystals are present, there are two things to be done, namely, to give alkalies, and to forbid and remove all that may become acid. For the first, caustic alkalies, carbonated alkalies and earths, saline draughts, and phosphate of soda;—these must be regulated according to the peculiar symptoms of the patient: for the second, vegetable acid, sugar, and starch in the food, should as far as possible be prohibited. The removal of acids by the skin, and of carbonic acid by free respiration and exercise, is of still greater benefit; shortly the uric acid crystals indicate that acidity must be removed, neutralized, and prevented.

"When uric acid crystals are absent, and urate of ammonia is deposited alone, it is a proof that no great excess of acid is present in the urine, otherwise uric acid would be found; and though by lessening the acidity of the urine, we can keep the urate of ammonia dissolved in it, and so hinder it from appearing, yet this is not the best mode of proceeding for effecting even this purpose. An extra glass of water, or soda-water, or some slight diuretic, as nitre, is more sure to keep the the urate of ammonia in solution by increasing the quantity of urine, and this is the best palliative treatment; whilst the curative treatment consists in lessening the quantity of food taken, by smaller meals, and in lessening the acidity of the urine, by increased exercise. Thus you will best prevent deposits of urate of ammonia, and the alkalies may be kept until uric acid crystals appear, when they are as necessary, as in the urate of ammonia deposits they are unnecessary, for the treatment."

Albuminous Urine.—[In commencing his observations upon this subject, Dr. Jones says:]

Heat and nitric acid together form the best, and indeed the only safe test for albumen, and the above is the order in which they should be employed. Either alone may give rise to mistakes. Dr. Jones very properly insists upon the importance of using a perfectly clean tube. A small quantity of strong acid will prevent the coagulation of the albumen by heat; and it even appears, that the same test may fail when the urine is very acid, without anything having been added to it. Albumen is never present in healthy urine, but by itself it merely indicates excessive congestion of some part of the urinary organs; to determine what disease is really present, the microscope must also be employed. By this means, we detect the presence of blood-corpuscles, or pus-globules, or fibrin. If the fibrin be moulded into the urinary ducts of the kidney, and if albumen be also present, with or without blood-globules, there must be congestion of the cortical structure of the kidneys; and most probably the disease is Bright's disease, or the result of scarlet fever. If blood-globules be found, and no fibrinous cysts, it is most probably that the congestion is not in the secreting structure of the kidney; and if crystals of uric acid or oxalate of lime be seen, then probably some calculus is causing abrasion of the vessels of the mucous membrane. Whether the calculus is in the pelvis of the kidney, the ureter, the bladder, or prostate, must be determined by the general symptoms. The nature of the epithelium that occurs in the urine, will also sometimes help to determine this question. The occurrence of the blood only on exertion tends to confirm the diagnosis of a calculus. If pus-globules be found with albumen, then suppurative inflammation is going on; if, at the same time, fibrinous moulds are seen, then probably Bright's disease and inflammation of the pelvis co-exist. If there be no moulds, but blood-globules and pus-globules, then probably a calculus has caused not only abrasion, but inflammation of the mucous membrane. When fibrinous moulds, blood-corpuscles, pus-globules, albumen, and crystalline deposit were seen, then degeneration of the kidney, inflammation, and calculus, were found on post-mortem examination. In one case, Dr. Jones found the urine albuminous from spermatozoa.

A peculiar substance, closely relating to albumen, has also been found in the urine, as well as in the buffy coat of inflamed blood. It is not precipitated directly by heat, nor by nitric acid, but if, after boiling, the urine is cooled, the precipitate falls, and is re-dissolved by heat. The nature of this substance is detailed by Dr. Jones in the 'Philosophical Transactions' for 1847.

Our author then relates the following interesting case: "The patient had for some time been passing the so-called chylous urine; but I have fully satisfied myself that the presence of fat in the urine, on which the white colour depends, is an accident only, and by no means the most peculiar part of the disease. The fat passes off in the urine, making it milky only after food; but previous to any food, albumen, fibrin, alkaline salts, and more rarely a few blood-globules, are thrown out by the kidneys, and their appearance in the urine depends on the increased force of the circulation, consequent on waking, on getting up, and on exertion; for, during sleep, the albumen, fibrin, &c., entirely disappear from the urine, and, by directing the patient to lie in bed, or to get up and exert himself, totally different kinds of urine were passed. When perfectly quiet, the albumen and fibrin were prevented from appearing in the urine; and, when he exerted himself before breakfast, these substances transuded in very considerable quantity. The urine sometimes, previous to any food, even formed a jelly-like clot, transparent, and perfectly free from the appearance of fat. I satisfied myself that the disease does not depend on chyle or on fat, but on a slight alteration in the state of the kidney, and on the circulation of the blood through it. Under treatment, the urine became perfectly healthy."

Diabetes and Diuresis.—The best tests for sugar in the urine, are the oxide of copper and oxide of silver. To a drachm of suspected urine, add two or three drops of a saturated solution of sulphate of copper, and then two drachms of caustic potash; the hydrated oxide of copper, which first is precipitated, re-dissolves if sugar or many other organic substances are present, becoming of an intense blue; and when heat is applied, if grape-sugar be present, the oxide of copper is rapidly reduced, and reddish-yellow sub-oxide of copper is precipitated. This reduction is effected by the carbon of the sugar. With cane-sugar, the changes do not take place until after a long application of the heat; and, under these circumstances, both uric acid and albumen will also effect a slow reduction of the oxide of copper. The rapidity of the change is consequently the test.

If silver be used, the following is the method to be employed. A few drops of a saturated solution of nitrate of silver are to be put in a test tube, and a single drop of caustic ammonia added; if a brownish oxide of silver fall, a single drop of the suspected urine is to be added, and the test-tube then heated, and its contents well shaken. In a few seconds, the sides of the tube will be coated with silver, and the metallic lustre will be seen.

The theory of the disease is as yet unknown. Sugar, as we saw before, is constantly formed in the body from starch; from some cause, with which we are not acquainted, the progress of the starch may be arrested at this point. In cases where the urine is excessively acid, the changes have advanced another step, as thus:—

In health the changes are, starch, dextrin, sugar, vegetable acid, caronic acid.

In excessive acidity; starch, dextrin, sugar, vegetable acid.

In diabetes; starch, dextrin, sugar.

But the starch taken in food cannot account for all the sugar in diabetic urine; for when the disease is fully established, total abstinence from starch and sugar will not cause the sugar entirely to disappear. Dr. Bensch has shown, that sugar of milk is invariably present in the milk of carnivorous animals fed exclusively on meat; and by boiling with water, the livers of animals fed exclusively on flesh have furnished positive proof of sugar.

There is a form of diabetes to which the term *insipidus* has been applied. In these cases, the urine contains a substance closely allied to sugar, but tasteless, and which may be converted into grape-sugar by the action of acids. In some cases of the ordinary disease, the urine has been found to contain these two saccharine bodies alternately; and therefore the term is a needless one.

The Relation of the Urine to the Food and the System.—[On this subject and the general method of its examination, Dr. Jones remarks:]

Any organ that is used must be repaired, and the substance that has been used must be removed. Take the muscles for example; the muscles consist of water, salts, non-nitrogenous fat, and a highly compound arrangement of carbon, hydrogen, nitrogen, oxygen, sulphur, and phosphorus. Carbonic acid, ammonia, water, sulphates, and phosphates are the last products of muscular action, and of the action of oxygen on the muscle. The intervening products, probably, are innumerable; as kreatin, kreatinine, uric acid, urea, choleic acid. Some of the products are thrown out of the body by the lungs, others by the kidneys. If the removal of some of these products by the lungs is stopped, the circulation through the lungs ceases in two minutes; the functions of the heart and brain are arrested, and from the mechanical stoppage in the lungs, death ensues. If their removal by the kidneys is stopped, in two days the patient is poisoned; the nerves and muscles are affected by the poison, and chemical death ensues. "If beef-steaks (the muscles of an ox) are given to one who has taken strong exercise, and is in perfect health, they are dissolved, and pass into the blood, and their chief use is to repair the muscle and nerves, not to form uric acid and urea, the constituents of the urine. The waste of the muscles, and other organs, passes off in the urine, whilst the food nourishes the wasting organs. Such I conceive to be the clearest ideas I can give you of the relation of the urine to the system, and to the food; and, theoretically, I consider this is the true healthy relation, and perhaps, in a state of full bodily labour, when enough food, and no more than enough, is taken, this may be the only relation; but provision has been made for too little labour and for too much food. If too much food is constantly taken, and too little exercise, plethora and hemorrhage must take place, if some escape for the excess of food be not provided. You have seen that the phosphates, urates, and sulphates are generally increased in the urine after food has been taken. If more food is taken than is required for the wants of the system, the excess is thrown out by the same organs that remove the waste of the muscles and other structures. If even excess of water alone is taken, the excess is thrown out partly, at least, by endosmotic laws not yet clearly applied. How the quantity of substances to be thrown out is determined, I do not yet distinctly see."

The great agent in effecting these changes is oxygen. Of this there are many familiar proofs, as the production of carbonate of potash in the urine after the citrate of potash has been taken. "Very lately, Professor H. Rose, of Berlin ('Phil. Mag.,' July, 1849) has made some most interesting experiments on the inorganic constituents of organic bodies, chiefly as regards their degree of oxidation. He divides the degrees of oxidation into fully oxidized, partially oxidized, and unoxidized. He compares the food, the blood, the flesh, and the urine. The food, if it consists of wheat and other grain, contains organic substances, the inorganic constituents of which exist partly in an oxidized, partly in an unoxidized state. Vegetable food, then, is partially oxidized. The blood is a partially oxidized body. The flesh is a partially oxidized body; but the quantity of unoxidized matter in the blood is larger than in the flesh, and the quantity of fully oxidized matter is smaller in the blood than in the flesh. The urine is a perfect and fully oxidized substance. The inorganic constituents of the urine are as highly oxidized as it is possible for them to be."—*London Journal of Med. Science, Oct.* 1850, *p.* 957.

55.—*Hypertrophy of both Kidneys in a New-born Infant.* By Rawson Senior, Esq., Batley.—[On the termination of a natural labour, Mr. Senior found that the infant, a full-grown male child, was in a state of complete asphyxia, which he supposed arose from the pressure of the umbilical cord, which was firmly entangled about its neck. All attempts at resuscitation were useless, as not the slightest action of the diaphragm took place. Upon manipulation, two solid indurated bodies were detected occupying the hypogastric and iliac regions, and presenting the appearance anteriorly of an unusually large liver; and laterally from the bulging out of the abdominal parietes, and from the position in the lumbar regions, indicating the probability of their being enormous kidneys. On a post-mortem examination being made, he says:]

The first thing attracting our notice was the close proximity anteriorly of the lateral edges of the two bodies, yet that they were contained in separate and distinct sacs. The one to the right was removed first, and it proved to be a kidney of immense size; it was weighed, and the weight of it was exactly *eight ounces.* The left one was not taken out of the house for the purpose of weighing; but, from its appearance, it could not have differed more than a drachm or two in weight. The supra-renal capsule was no larger than usual; and the investing tunic, though somewhat denser than we commonly find to be the case, was easily detached. The kidney was composed of numerous lobules, some large, others small; the larger ones forming the outer boundary, and the smaller ones occupying the notch or fissure; altogether giving it the shape of an irregular oval body. When laid open, a quantity of urinous fluid oozed forth from the tubules, but their appearance presented nothing remarkable, being of a palish red colour, as is found to be the case in the kidney in its normal condition. The blood-vessels, proportionally larger in the fœtus than in the adult, as we well know, were found to exceed that proportionate increase in size to some considerable extent, the emulgent vein especially. The ureters retained their accustomed size, and the bladder, liver, and the other abdominal contents, had a healthy appearance.

Upon a review of this singular case, we cannot but observe the disproportionate arrangement which existed between the kidney and its supra-renal capsule. I believe that we generally find the supra-renal capsule to a third the size of the kidney itself at birth; but here it did not correspond with the enlarged organ. But it would appear that compensation had been afforded to the blood-vessels, their calibre being much enlarged, and the great weight and remarkable size of the kidneys were doubtless inevitable results of the increase of development of the vascular system of the kidney,—the vital feeder of this important secreting and excreting structure; so that we may view the case as one of those singular freaks of nature which are now and then produced, demonstrative alike of her influence and agency in overburdening as well as nicely adapting organs so highly essential to the economy and integrity of the functions.—*Medical Times, Sept.* 7, 1850, *p.* 265.

56.—*Strychnia in Paralysis of the Bladder.* By M. Lecluyse, of Toperinghe.—M. L. was called to a man of 68 years of age, who, after a day of excess, had retention of urine, which led to complete paralysis of the bladder. After trying all the ordinary methods, such as permanent catheter in the bladder, copaiba, turpentine, stimulating diuretics, cold douche, and cantharides, he gave strychnia internally. It was pushed as far as one grain in the day, and produced tetanic spasms in the muscles of the trunk and limbs, but left the bladder as thoroughly paralysed as ever. After fifteen days' abstinence from all treatment, he resolved to apply the strychnia in the form of injection into the bladder. For this purpose he dissolved six grains in alcohol, mixed this with a pound of water, and ordered four injections of two ounces each to be introduced through the catheter, after previously emptying the bladder. At first the bladder seemed insensible to the contact of the liquid, but after four or five days, the patient found himself wet with urine, though the fixed catheter was well closed, and he supposed that this was due to a fissure in the instrument. It was withdrawn for the purpose of being examined, and to the astonishment of both patient and surgeon, the urine flowed freely and well, and the patient recovered completely, with no other inconvenience than some momentary irritation, probably caused by the irritation of the catheter.

M. L. thinks himself warranted, from the length of time that elapsed between the internal administration of the strychnia, and the application of it by injection into the bladder, in concluding that the former had nothing to do with the cure.

[The following case will show how strychnia acts when injected into the bladder in a soluble and therefore absorbable form.]

A man, aged about 48, of good constitution, had paralysis of the bladder consequent upon a debauch. He remained three days without treatment, and the medical man who saw him, finding the urine dribbling away, mistook the case for one of incontinence of urine, and prescribed some trifling remedies. The patient was admitted into the Hôpital Beaujon under M. Robert, who finding that besides retention from paralysis, there were symptoms of cystitis, such as pain in the hypogastrium and kidneys, and purulent urine, ordered a bath, leeches to the perineum, and the use of the catheter twice daily. Having thus relieved the more urgent symptoms, he proposed to adopt M. Michon's practice of applying electricity, but not having an apparatus at hand, deferred it. In the meantime, M. Lecluyse's plan of injecting strychnia having been mentioned to him, he resolved to try it, but was advised to use it in smaller doses. He ordered three grains (twenty centigrammes) in six ounces of water. The strychnia not being soluble in this amount of water, the pupil in charge acting upon the principle—*corpora non agunt nisi soluta*—substituted sulphate of strychnia for the alkali itself. The injection was given; in a few minutes afterwards, the *interne* who had charge of the case, observed convulsive movements coming on, with opisthotonos, and especially spasms of the thoracic muscles, which alarmed him much. The bladder was immediately emptied, and washed out with tepid water. The symptoms did not become worse, they gradually diminished, but for some days the patient experienced spasms of thoracic muscles. The most remarkable fact is, that in spite of the severity of these symptoms, which indicated a great intensity of action on the part of the strychnia, the bladder did not recover its tonicity, but remained paralysed. The only appreciable change in this respect was, that it was remarked on the third day, when the catheter was introduced, that the urine flowed with a little more force than before, but this was a very insignificant amelioration. The spasmodic affection of the chest continued, the patient was seized with bloody expectoration (black mucous sputa, differing from those of pneumonia, and appearing to result from a sort of asphyxia), symptoms of urinary fever soon came on, and the patient died. There was no dissection.—*Bulletin de Thérap.*—*Monthly Journal of Med. Science, Aug.,* 1850, *p.* 176.

57.—*Electricity in Paralysis of the Bladder.* By M. MICHON, Surgeon of La Pitié.—[According to M. Michon's cases, electricity in this condition of the bladder, appears to be an efficacious remedy. The first case is that of a pedlar, aged 67, who was admitted on the 9th of October. It is stated, that after eight days' moderate wine drinking, although in apparent good health, he was disturbed in the night by a desire, but total inability, to pass water. A surgeon introduced the catheter, drew off the urine, ordered twenty-five leeches to the perineum, and stimulant frictions, but for eight days the catheter was needed night and morning.]

When admitted under M. Michon, he had absolute impossibility of passing urine without the aid of the catheter, had no fever, and no pain beyond the uneasiness caused by the distension of the bladder. The day after admission the bladder was again emptied by means of a large catheter, which passed freely. There was no kind of stricture or obstruction, and the prostate was ascertained by the finger to be sound; The urine flowed from the catheter with a feeble jet, which gradually diminished as the bladder became emptied. It was necessary, in order to evacuate it, to make pressure on the hypogastric region. The patient's account of himself did not tend to show that there had been any previous want of power in the bladder; but M. Michon was inclined to suppose that it had been gradually losing tone from the man's advanced life, and that complete paralysis had followed from over distension on the night of his seizure. He was ordered demulcent drinks, laxative enemata, and the catheter twice daily. Two days after, the patient having suffered much from repletion of the bladder, M. Michon left the catheter in, to enable it to be evacuated more frequently without risk of injury to the urethra from less skilful hands. Injections of tepid water into the bladder, discharging the urine by the catheter every three hours, Vichy water, blister to the hypogastrium, cantharides liniment to the belly and thighs, were all tried without success. On the 6th November the patient had rigors and vomitings; the urine became turbid and ammoniacal, with a copious puriform deposit. The catheter was removed, and the febrile symptoms relieved by baths and emollient poultices. The urine remaining ammoniacal, acidulous injections were used, with the effect of slightly improving it, but the retention was as complete as ever. As the patient, towards the end of November, lost ground, M. Michon resolved to have recourse to other means, and tried the effect of electricity.

The method of applying the electricity was as follows:—A silver catheter was introduced into the bladder, and a female catheter introduced into the rectum, and made to rest upon the recto-vesical parietes; each of these catheters was connected with one of the poles of an electric machine, which was put at first in gentle action, and then continued briskly for two or three minutes. The patient experienced no pain, he felt only some slight prickling; the urine had to be drawn off twice in the succeeding twenty-four hours. The electricity was re-applied next day in the same manner,—the patient felt the prickling more acutely, but only in the rectum,—he felt nothing in the bladder. A third application of the electricity was made, the same sensations were experienced, but, as on the two former occasions, ceased immediately afterwards. On this night he felt a desire to make water, and accomplished it easily without the aid of the catheter. Next day he was up, and showed the urine which he had passed during the night. From that time he required no more catheterism, but the electricity was applied again for three successive days; it produced the same effects, only that the sensations were more acute, and that during its application the urine was projected from the catheter at each electric stroke. During the intervals the urine was passed at his will, and without difficulty; it rapidly lost the ammoniacal odour, and became limpid. The fever disappeared, the patient regained his strength, and after remaining in the hospital for three weeks without any urinary disorder, was discharged cured. He presented himself again in three weeks, at the request of Dr. Michon, and was found to be quite well. It is proper to state, that during the application of the electricity the catheter was not allowed to remain in contact with one single part of the bladder, but was moved gently over the inner surface of the organ.—*Monthly Journal of Med. Science, Aug.*, 1850, *p.* 173.

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SURGERY.

DISEASES OF JOINTS, FRACTURES, ETC.

58.—ON SCROFULOUS DISEASE OF THE JOINTS.

By Sir B. **Brodie**, F.R.S., &c.

[In Sir B. Brodie's new edition of his work, on 'Diseases of the Joints,' we find the following practical remarks on scrofulous disease of the hip-joint:]

"The practice which has appeared to me to be, on the whole, the best, is the following. An opening having been made with an abscess-lancet, the limb may be wrapped up in a flannel wrung out of hot water, and this may be continued until the first flow of matter has ceased, a poultice, or water dressing, being applied afterwards. In some instances, after a short time the discharge ceases; the orifice heals, and the puncture may then be repeated some time afterwards. But where the puncture has not become closed, I have never found any ill consequences to arise from its remaining open. On the contrary, I have no doubt that it is desirable that the wound should not be closed until the abscess has contracted, granulated, and healed from the bottom; and this is one reason for making, not a small puncture, but a free opening with an abscess-lancet. Another reason is, that the matter will escape readily without squeezing or pressure. *All rough manipulation is to be carefully avoided.* It produces hemorrhage into the cavity of the abscess, the ill consequences of which I have already pointed out; and, independently of this, it may excite inflammation of the cyst, attended, where the surface is extensive, with so much constitutional disturbance as to endanger the life of the patient at the time, and materially lessen the chance of his recovery afterwards.

"The treatment of the sinus which is left after the opening of an abscess may be comprised in a few words. If the orifice be disposed to heal prematurely, this may be prevented by the occasional application of the caustic potash, care being taken that the caustic does not enter the sinus itself; otherwise some simple ointment or a water dressing is all that is required. The old practice of probing a sinus scarcely ever affords us any useful information; nor does it in ordinary cases answer any other good purpose. On the other hand, by irritating the sinus, or even the joint itself, it is often productive of serious mischief. The same observation is applicable, but with greater force, to the use of stimulating injections. I do not believe that they promote the healing of sinuses under any circumstances; but, with respect to those which are now under our consideration, there is no doubt that their operation is highly injurious. I saw a young man who nearly lost his life, in consequence of a surgeon having ventured to inject port wine into a sinus connected with some diseased or dead bone of the pelvis."

In the progress of this disease there are evidently several successive stages. The author is of opinion, that if the disease have not proceeded to suppuration, the patient may recover with a serviceable and moveable joint, even when a considerable portion of the cartilage has disappeared. Otherwise there is no recovery, except with anchylosis. The anchylosis is at first affected by soft substance. But afterwards perfect bony union takes place; so that when the bones composing the joint are divided by section, the cancellated structure of the one bone is seen continuously passing into that of the other.

The determination of the question of amputation in instances of this species of articular disease, Sir Benjamin Brodie makes to depend upon the point, whether or not there be indications of other tubercular disease in any of the internal organs. It too frequently happens, that when one diseased joint is removed by amputation, another becomes the seat of the same malady, or tubercular deposits in the lungs, the bronchial glands, or in the mesentery, finally destroy the patient. When there is any reason to apprehend such results, amputation, of course, is worse than useless.

[Speaking of the relations, character, and treatment of chronic diseases of the joints, connected with gout and rheumatic gout, Sir Benjamin remarks:]

"I have stated that inflammation of the synovial membranes, as it presents itself under ordinary circumstances, may not unfrequently be traced to a gouty diathesis. It is so in numerous instances, in which, nevertheless, neither the patient nor his medical attendant would admit that there had ever been what is called 'a fit of the gout.' I have referred to rheumatism as a cause of inflammation of the synovial membranes in other cases, and I have also adverted to a peculiar disease, of a very chronic character, which is generally supposed to bear some relation to both gout and rheumatism, and treated of under the name of rheumatic gout. In this disease, inflammation of the synovial membrane is the first of a series of changes which the joint undergoes, and which, in the course of years, ends in its entire disorganization.

"In the museum of St. George's Hospital there are many other specimens of joints similarly diseased. From these we learn that the lithate of soda is deposited in a variety of textures:—underneath the synovial membrane, on the bone near the margin of the cartilage,—on the surface and in the substance of the cartilage; in the cancelli of the bone, and in the cellular tissue external to the joint. In one preparation of the patella, taken from a gouty subject, the cartilage is seen increased in thickness, and presenting a striated appearance, the striæ being at right angles to the articulating surface. In this case the cartilage is described in the catalogue as having been 'in its recent state softer and more yielding than natural, and having a tendency to break up into fibres.'

"Although the opportunities of examining the pathological condition of joints which are affected in this manner are only of occasional occurrence, there is no surgeon of much experience who has not seen many cases of the same disease in the living person, or who, having seen them, will not assent to the correctness of the observation which I have already made, that inflammation of the synovial membrane is the first of the series of changes which the joint undergoes. This is clearly indicated by the symptoms. The deposit of lithate of soda takes place only as the disease advances; and though it is a very frequent, it is not a constant, occurrence. The absence of this deposit is not a proof of the absence of the gouty diathesis. An elderly gentleman was attended by Dr. Chambers and myself, labouring under well-marked symptoms of gouty disease. One of the effects produced was a chronic inflammation of the synovial membrane of one knee. The membrane was considerably thickened, and contained fluid. He had been in this state, sometimes better, sometimes worse, for some years. When living at his house in Scotland, he was seized with symptoms of acute phrenitis, of which he died. The body was examined by his medical attendant, Mr. Johnstone, who found the diseased joint distended with a dark-coloured glairy fluid, and the cartilages absorbed, but without any white incrustation.

"Indeed, in cases of gout generally, it is difficult to discover any rule by which the deposit of lithate of soda is regulated. One person had his fingers and toes distorted by gouty concretions, at the same time that deposits of the same kind are found in the larger joints, and in the subcutaneous cellular tissue in various parts of the body; while another person, who has been equally tormented by the gout with the former, is altogether free from this additional cause of distress. Probably much depends on the greater or less prevalence of soda in the system, the lithate of soda being an insoluble salt, and in this respect differing from the combinations of the lithic acid with the other alkalies. There is great reason to believe that many of those who are troubled with what are commonly called chalk-stones have much aggravated, if they have not actually produced the evil, by taking large quantities of soda for the purpose of correcting acidity of the stomach.

"The process by which the absorption of the harder textures takes place in these cases is manifestly very different from ulceration, and is altogether very remarkable. The cartilages disappear, so that the bones are exposed; but the latter present nothing corresponding to the appearance of a carious surface. They bear evident marks of having been subjected to the influence of friction, appearing as if portions of their surface had been scraped off by a chisel. This is especially observable in the knee, where the motions of the joint are only in one direction, and less observable in the hip, where the motions are more various. But the results of friction on the living are very different from what they would be in the dead bone. There is no exposure of the cancellous structure; a process of repair goes on simultaneously with that of destruction, and the consequence is the formation of a hard and compact layer of bone, bearing no small resemblance to ivory, covering the cancellous structure, which must have been exposed otherwise.

"In one of the cases which have been just described, it was found that some new formations of bone, in the shape of small exostoses, had taken place at the margin of the articulating surfaces. I shall have occasion hereafter to refer to two other cases, in each of which portions of such newly-formed bone had been broken off by the motion of the joint, and become loose bodies in the articular cavity. In other cases, masses of new bone are laid on in one part, while the original bone is removed by absorption in another, so as completely to alter the form and character of the articulating surfaces.

"*On the symptoms of this disease.*—The individuals who are most liable to be thus affected by rheumatic gout, are those who have not been accustomed to much bodily exercise; and have at the same time led luxurious lives. I suspect that too great an indulgence in the use of animal food is even more likely to produce it than the free use of fermented or spirituous liquors, and that it is for this reason that it is frequently met with among porters and the upper servants of wealthy families.

"The disease is rarely confined to a single joint, and in most instances several joints are affected in succession. Often it shows itself in the first instance, in a joint of one of the fingers, which is observed to be slightly enlarged and stiff, with an occasional twinge of pain in it. Then, one after the other, other joints of the fingers are affected in the same manner. It was to this enlargement of the joints of the fingers that Dr. Haygarth gave the name of *nodosities*. The immediate cause of it seems to be a thickening of the synovial membrane, and probably in part an effusion of serum into its cavity. Afterwards the disease extends to the other and larger joints, sometimes to a greater, sometimes to a smaller number in succession. In those joints which are superficially situated, so as to admit of examination, fluid may be detected, the quantity of which, however, varies accordingly as the limb is exercised or kept quiet, and is seldom very large The fluid is more perceptible in the first instance than it is afterwards, on account of the synovial membrane becoming thickened.

"The progress of the disease is generally very slow, so that many years may elapse before it reaches what may be regarded as its most advanced stage. Throughout the whole of its course, it causes no severe pain, but there are constant uneasy sensations, and much distress and inconvenience, in consequence of the joints becoming gradually more rigid and unfit for use. The patient, in addition to his local ailments, always suffers, in a greater or less degree, from the usual effects of dyspepsia, which are aggravated by want of exercise. He is liable to acidity of the stomach, and flatulence after his meals; is nervous and irritable; and every error as to diet, as well as all mental excitement, will produce an aggravation of both the constitutional and local symptoms.

"In a few instances, after having reached a certain point, the disease becomes stationary, or there may be apparently some degree of improvement. But except where it was treated in the very earliest stage, I do not recollect any one case in which there was anything approaching to a cure. In the majority of cases, the disease is progressive, the joints becoming more and more disorganised, sometimes with little, at other times with much, increase in size. That increase, in this stage of the disease, depends not merely on the causes already mentioned, but also on a deposit of new bone in the neighbourhood, and sometimes on that of a gouty concretion in the surrounding cellular tissue. Anchylosis sometimes takes place as an ultimate result. There is little tendency to suppuration. I cannot say that abscess never forms, for I suppose that there are no morbid changes of any kind, in the course of which this may not possibly occur; and, indeed, in one of the cases which I have just recorded, it was believed that there were pus-globules mixed with the semifluid substance which the joint contained. But the formation of abscess in these cases certainly does not belong to the regular order of events, and must be regarded as a rare exception to the general rule. This is a remarkable circumstance in the history of the disease, and forms an important difference between it and the ordinary diseases to which the joints are liable.

"Distressing as such a malady must always be, harassing the patient as it does, year after year, with no prospect of a cure, still it is an evil varying very much in degree in different instances. In one individual, a few joints only are affected, and these not to such an extent as to prevent him from taking a certain amount of exercise, and enjoying the advantages of social intercourse; in another, scarcely one joint of the extremeties remains in a sound state; some are completely anchylosed, and others are so stiff as to be nearly useless. Nevertheless, the patient thus afflicted, a cripple, dependent on others for the means of locomotion, may live for years, reconciled to calamities which have gradually come upon him, and in the possession of a certain amount of comfort amid all his sufferings.

"*On the treatment of this disease.*—In the very commencement of this disease, before any actual disorganization has taken place, and while the joints affected are limited in number, I believe that much may be done towards preventing its further progress. The patient should be placed on a careful system of diet, partaking very moderately of animal food, avoiding fruits, acids, raw vegetables, and sugar, and taking little or no fermented or spirituous liquors. He should take exercise daily, so as to induce perspiration; and, if this cannot be readily accomplished, he may, with great advantage, once in a week or fortnight, make use of the hot-air bath. In addition to such careful management of himself, he may from time to time take alterative doses of the acetic extract of colchicum, combined with a small quantity of the mercurial pill, and occasional purgatives. Moderate doses of potash or magnesia may be given three or four hours after each of his principal meals, so as to neutralize any superabundant acid in the stomach. Soda should be carefully avoided, as tending, by its combination with the lithic acid, to form gouty concretions.

"According to circumstances, however, the treatment may be varied. Thus, when the patient is depressed, as sometimes happens by the use of colchicum, the mixture, composed of rhubarb, magnesia, and ginger, sold under the name of 'Dr. Gregory's Powder,' may be taken every night, with an active aperient at stated intervals, and very great benefit will often be obtained from a very long perseverance in the use of these simple remedies.

"Little is to be done by local applications. If, however, there be more than usual pain in a point, leeches may be applied, and, on particular occasions, a bandage, not for the purpose of making pressure, but of limiting motion. In some instances, a light leathern splint, or pair of splints, may be employed with advantage.

"When the disease is fully established, the same kind of treatment will be useful in mitigating its symptoms, and sometimes in retarding its progress. But here, the more active remedies, such as mercury and colchicum, must be reserved for special occasions. The iodide of potassium has the reputation of being useful in cases of this description; and I believe that its reputation is not wholly undeserved. It should be given only in small doses of two or three grains twice daily, but taken if it agrees with the patient, for several weeks at a time.

"But, after all, no general rule can be laid down as a guide for the practitioner on all occasions. Each individual case forms a study in itself, not only for the medical attendant, but for the patient also.

"In one case, in which the patient was afflicted with this disease in a very aggravated form (so many joints being affected, that she lay almost helpless on a sofa, and various methods of treatment having being already employed to no purpose), having learned that cod-liver oil had been formerly supposed to be useful in some forms of rheumatism, I was tempted to prescribe this remedy for her. The purified oil was administered internally, and at the same time rubbed in as a liniment on the affected joints. It was a mere experiment; but there is certainly great reason to believe that it was not altogether unsuccessful. Not only did the disease cease to make progress, but, when I last saw the patient, after she had taken and rubbed in the oil, with occasional intermissions, during a period of seven or eight months, there was a distinct and manifest improvement as to all the symptoms. Indeed, the result was such, that, to any other patient under the same combination of circumstances, I shall certainly be disposed to recommend a trial of the same remedy."
—*Edin. Med. and Surg. Journal, Oct.* 1850, *p.* 445.

59.—ON DISEASES OF ARTICULAR CARTILAGES.

By Dr. Redfern, Examiner at the University and King's College, Aberdeen, &c.

Dr. Redfern ventures to advance what will to many appear a most improbable supposition,—namely, that the *pain* which is commonly accounted an indication of disease in articular cartilage, is not to be so regarded; but that it is to be attributed to the implication of some other texture. This is a very important point in diagnosis. Dr. R. says:

"Now, if it be true, that disease having the same essential structural characters in all cases, at times goes on to the complete destruction of the texture, without the patient or his attendant being aware of its existence; and in others, causes such violent pain and hectic irritation in the system as to require amputation, the fact is of extreme importance, as, so far as I know, no similar occurrence takes place in any other texture whatever.

"I shall not presume to deny, that uncomplicated disease of articular cartilages produces excessive pain, since this is stated very positively on the high authority of Sir B. Brodie, and by most surgical writers. And yet, on looking over the numerous cases which have been related by Sir B. Brodie and other authors, I find but few which will aid in the determination of the question. Uncomplicated disease of cartilage is of very rare occurrence, if we exclude the cases of fibrous degeneration, and carefully collected reports of numerous cases, with histological examinations, are necessary to establish a fact so decidedly at variance with everything we know of disease and its characters in other textures. This will appear more evident from the following considerations:

"1st. Nerves have never been found in cartilages, and there is not the slightest reason for believing that they contain any, for they give no evidence of sensibility in man or in animals on being exposed or sliced away. The cuticle, enamel, and the substance of the brain are in a similar position. They contain no nerves, and give no indications of sensibility, when parts are removed, or otherwise destroyed. Moreover, it is well known to every pathologist, that an immense portion of the whole bulk of the cerebrum may be utterly destroyed by disease, without a single recognised symptom or other mark of the existence of the lesion. Hence the value of pain.

"2nd. It is certain that disease of cartilages may go on for years, and even be cured by the efforts of nature, without the knowledge of the patient, and that the essential characters of such disease are similar to those of other diseases of the same tissue, which are supposed to be the cause of excessive and exhausting pain.

"3rd. The symptoms supposed to indicate disease in the cartilages are so closely allied to those produced by affections of the bones, that a positive diagnosis is very difficult to make until the bone becomes exposed, and then the symptoms are, at least, as easily attributable to the disease in the bone as to that in the cartilage. Indeed, M. Richet, in the work before named, when entirely ignorant of the real nature of the changes which take place in diseased articular cartilages, gives several good anatomical and other reasons why the symptoms supposed to arise from disease in them should be referred to the bones.

"4th. Pus has never been shown to be formed from the substance of diseased articular cartilages, nor from effusions into their texture. At the time when pus is formed in joints, the pain becomes very severe, though there may have been but little previously, the very formation of pus indicating that other textures than the cartilage are affected, and that the pain proceeds from the disease in them."

In order to test and extend the information derived from the observation of disease in man, Dr. Redfern has performed a series of experiments upon the cartilages of the lower animals; these experiments were continued during a whole year, and were nearly ninety in number; the progress of each one was carefully watched and recorded from day to day; and on the death of the animal, a lengthened microscopical examination was made of every texture in the neighbourhood of the parts operated on. The results of similar experiments were always found to correspond, when performed upon animals of the same species, in all cases of which some accidental occurrence had not taken place, such as the supervention of acute inflammation in the joints, pressure on the articular surfaces resulting from dislocations, &c; and thus they admit of being generalized with considerable precision. We must forbear entering, however, into the minuter differences which distinguished the results obtained on the cartilages of the ribs, the sternum, the ear, the nasal septum, and the joints, when these were subjected to irritations of various kinds, such as the making of incisions into them, the insertion of setons, the application of ligatures, actual cautery, acids, or alkalies; but shall cite Dr. Redfern's account of the phenomena common to all.

"When changes are induced in the costal, articular, or nasal cartilages, by any cause whatever, they are essentially similar in all cases, and affect both the cells and inter-cellular substance. The cells become enlarged, rounded, or oblong, and irregularly arranged; their nuclei giving place to a multitude of corpuscles, or to a series of granules. The walls of the cells next become identified with the hyaline substance, when the corpuscles are set free, and begin to undergo other changes, elongating and becoming very evidently converted into the nuclear fibres of fibrous tissue, or reduced to small patches of granules, which may only become visible on acting upon the fibrous mass by acetic acid. During the enlargement of the cells, and especially at the period of discharge of their contents, the hyaline matrix becomes softened, and allows the corpuscles to be freely moved in it, or pressed out altogether; whilst, at a subsequent period, fibres appear in the interspaces of the corpuscles, and take on the characters of those of the white fibrous tissue. In this way, both elements of the cartilage are concerned in the formation of the fibrous membrane, which is destined to heal the breach in the tissue, for the hyaline substance is converted into the white fibres, and the corpuscles of the cells into nuclear or elastic fibres.—Or, an altogether different result may take place, especially in the case of articular cartilages, the whole thickness being converted into a calcareous mass; the cartilage cells becoming lacunæ, and calcareous matter being also deposited in the inter-cellular substance, rendering it dark and incapable of transmitting the light, the surface all the while remaining perfectly smooth and on its former level."

This production of a dense fibrous membrane, or of a smooth calcareous lamina, appears to be nature's method of effecting a spontaneeus cure; the tendency to which is very decided in uncomplicated lesions of cartilage, especially in such as have been artificially induced. In no case do these changes seem to extend further into the healthy cartilage surrounding an injured spot, than appears necessary for the formation of sufficient fibrous texture to constitute the cicatrix. It is a remarkable result of Dr. Redfern's inquiries, that these changes occur much less rapidly after incisions, than they do after great irritations; so that even after seven weeks, the texture of incised cartilages presented very trifling alterations. Hence the belief has arisen in the minds of many observers, even Dr. Leidy being among them, that the two surfaces of an incised cartilage never unite. In Dr. Redfern's experience, however, it was always found that the changes consequent upon incision were of the same kind with those following upon irritation, although more tardy in their rate of progress; and that, if sufficient time was given, they would go on to the production of perfect fibrous tissue, and thus to the reunion of the divided surfaces. The fibrous tissue, which heals up breaches in the texture of cartilages, may contain (according to Dr. Redfern) the yellow as well as the white fibrous elements; the latter being formed by the elongation of the discharged corpuscles of diseased cells into nuclear fibres; whilst the former originate in the altered hyaline substance. All these results are in precise accordance with the inferences drawn from the study of the human articular cartilage in states of disease; and each set of observations thus affords a valuable corroboration to the other.—*Brit. and For. Medico-Chirurg. Review, July,* 1850, *p.* 177.

[The conclusions to which this learned author has arrived are as follow:]

1. That the whole of the known forms of disease in articular cartilages are connected with changes in the texture, which are essentially similar to each other.

2. That during the progress of these changes, the cells of the cartilage become enlarged, rounded, and filled with corpuscles in lieu of healthy cells, bursting subsequently, and discharging their contents into the texture on the surface; whilst the hyaline substance splits into bands and fibres, the changed hyaline substance and the discharged corpuscles of the cells afterwards forming, in many cases, a fibro-nucleated membrane on the surface of the diseased cartilage.

3. That these changes are alone referable to an anormal nutrition as their immediate cause, and in no case to mechanical or chemical actions, such as attrition or digestion in a diseased secretion.

4. That most extensive disease may be going on in many joints at the same time, and may proceed to destroy the whole thickness of the cartilage in particular parts, without the patient's knowledge, and whilst he is engaged in an active occupation.

5. That the disease commences most frequently upon the free surface; but it may proceed from the bone to affect the attached surface, or it may take place in the middle of the thickness of the cartilage.

6. That it is at least very doubtful, if the symptoms which are believed to indicate the existence of ulceration of articular cartilages, are not really dependent on a morbid change in the bone.

7. That disease of the whole thickness of an articular cartilage, at particular parts, admits of a natural cure, by the formation of a fibro-nucleated membrane from the substance of the cartilage, without the occurrence of any new exudation.—*London Journal of Med., June,* 1850, *p.* 568.

60.—*Hip-Joint Disease treated by Cod-Liver Oil, at St. Thomas's Hospital.* Reported by Dr. C. Kidd.—[Dr. K. having seen several cases of this disease improved under the use of cod-liver oil, thinks it of such practical importance as to deserve a few passing remarks. The general opinion of surgeons seems to be, that this disease is not of mere local origin, but indicative of defective bodily powers,—taking blood, even by leeches, at the commencement being often attended by evil results. Sea-air, tonics, especially the mild proto-salts of iron, given for a long time, with occasional intermissions, and cod-liver oil, are the means trusted to in London. In illustration of these remarks, Dr. Kidd describes three cases, and he says several others could be added, in which the benefit derived from the cod-liver oil treatment was highly satisfactory. Dr. Kidd, pointing out rest as one of the chief indications to be fulfilled, if possible, recommends that the patient should be put upon one of Earle's beds, and the greatest attention paid to his bodily comforts, so as to prevent restlessness. If the disease is so advanced that abscess has formed, of course we can look for nothing better than anchylosis, and even this it may take years to effect. Speaking of the treatment of abscesses about the hip-joint from this cause, Dr. Kidd remarks:]

In puncturing abscesses about the hip-joint, great care must be taken; all kinds of rude or rough surgery avoided; and the abscess allowed to empty gradually. Where anchylosis is all we can look for, much devolves on the surgeon in keeping the limb in the best position; for the patient, if left to himself, will infallibly bend it at a right angle. The proper position is, of course, that of the thigh bent a very small degree forwards,—the limb will be not only thus more useful, but less probability is incurred of its getting on the dorsum of the ilium. A simple gutta percha splint, embracing one side of the pelvis, about half the circumference of the thigh, and extending below the knee, is all the appliance necessary. When there is great pain, a seton or blister will be found useful.

As to the rationale of the *modus operandi* of cod-liver oil in this disease, perhaps the best way of explaining it, is by saying it changes that abnormal molecular condition of the blood so well known to exist in these cases, and so often connected with scrofula and other constitutional causes. To say there is ulceration of cartilages in some of the worse cases, as we long ago thought, would of course be at present a little heterodox. Cartilage under the microscope has no vessels, and consists essentially of true cells, as was long since shown by Schwann, and in this country by Mr. Toynbee; but that *diseased* cartilage is not vascular, is possibly open to doubt. Softening of the cartilage of the hip-joint is not uncommon, and forms, perhaps, one of the earlier states of the disease under inquiry. The cells become enlarged, softer, and contain nuclei; chondrin changes into gelatine—these changes being preliminary to what we will call ulceration—that scrofula, syphilis, rheumatism, often produce these changes is also very probable. Mr. Birkett and others explain these changes, I need scarcely add, by saying, "disintegration" takes place, in which the development of fat plays no unimportant part. In this observation, we do not, perhaps, get rid of the difficulty; and may ask, with Sir Benjamin Brodie, whether ulceration, in whatever part of the body it takes place, is not essentially a process of disintegration, depending on a want of proper vital nutritive power. If this be the truth, the operation of cod-liver oil is not such a puzzle. Iodine has not been found of as great use in so-called scrofulous disease of joints as iron—an idea which agrees with an opinion now gaining ground, that the former interferes with the protein-forming power of the system, and is not the active principle of the oil—from having an opposite effect.—*Med. Times, July* 27, 1850, p. 89.

61.—*Removal of the Head of the Femur at St. Bartholomew's Hospital.* By —— Skey, Esq.—Ann Sugg, æt. 13, fell over a skipping-rope about three years ago, and received some slight contusions about her left hip; inflammation of the joint followed: she was unable to use the limb, and suffered a great deal of pain in the knee. About a year after the accident she was able to limp about, but could only get the *toes* of her left foot to the ground, as she was unable to extend the knee or hip joints. In a few months abscesses formed around the hip, and burst; and for the last seven months some of them have remained open.

June 6th, 1850.—Admitted into St. Bartholomew's Hospital in an extremely emaciated condition, and nearly worn out with suffering. The left femur was dislocated on the dorsum ilii, the limb shortened, and the leg and thigh flexed: there was a large ulcerated surface over the trochanter major, through which the bone threatened to protrude, with burrowing sinuses in the neighbourhood discharging pus freely. After being in the hospital a few weeks the child gained a little flesh, but she has lately fallen off again, not being able to stand the pain and continued discharge of matter from the wound. It was considered that removing the head of the bone would give the patient the best chance of recovery; and the operation was performed on August 17th, by Mr. Skey, the patient being under the influence of chloroform. As the end of the bone was only thinly covered with granulations, a very little cutting sufficed to expose it; this being accomplished, the limb was carried inwards, and the bone divided with the saw just below the great trochanter. The granulations bled freely on being cut, but the hemorrhage soon ceased, and no ligatures were required. The integuments were too firmly adherent to the parts beneath to admit of being drawn together; the wound was therefore left open.

The acetabulum was found to have been enlarged by absorption, and was extended in a direction upwards and backwards, as if an attempt had been made by nature to form a new joint in this direction. The head of the femur had been entirely absorbed: a portion of the neck remained, which, with the great trochanter, was the part removed: on dividing this with the saw it was found to be soft, and composed of vascular cancellous structure, with a very thin outer layer of compact bone.—*Med. Gazette, Aug.* 30, 1850, *p.* 382.

62.—*Disease of the Elbow-Joint; Peculiar Method for obtaining speedy Anchylosis.* Under the care of Mr. Gay, at the Royal Free Hospital.—[The editor of 'The Lancet' observes that it is only by carefully collecting the facts bearing upon this important mode of treatment, that we can throw some light upon the result obtained, and for this reason we present the following case, under the treatment of Mr. Gay.]

The patient, whom we recently saw in the surgical ward, is twenty-nine years of age, of light complexion, and a rather nervous temperament. Four years ago he felt a stiffness of the elbow-joint, and an abscess subsequently formed in the under part of the fore-arm, about two inches from the elbow; it was opened, discharged for two months, and healed. The patient returned to his usual occupations, though the joint remained stiff, but about four months before admission, the joint swelled and became again painful upon the slightest movement. He was then treated as out-patient to the hospital, and a large abscess formed on the outside of the arm, about two inches from the elbow, which was opened by Mr. Jackson, the house-surgeon. It discharged a considerable quantity of thick, curdy matter, and, as the opening contracted, it was laid open again three weeks afterwards, when the patient was admitted into the hospital.

The joint was now enlarged, very painful, both when moved or compressed, and on rubbing the bones entering into the formation of the joint against each other, no doubt could exist but that the cartilages were gone. The patient's health was at this time much impaired, and the question of amputation was discussed. Mr. Gay preferred, however, adopting a plan which he has successfully employed in similar cases for some years past—viz., that of freely opening the joint by an incision along its outer side. This measure was at once carried into execution, the length of the incision being about two inches and a half. A large quantity of pus escaped, and the finger passed in through the diseased capsule could readily detect the rough and denuded ends of the humerus and ulna. The wound was filled with lint, and the arm lightly bandaged: the subsequent fever was very slight, and the discharge, which was rather abundant, continued about one week. At this time it considerably diminished, the fever abated, and the joint begun to be somewhat rigid. Mr. Gay prescribed tonics and a generous diet; directed water dressing to the wound, and the arm to be pretty firmly bandaged. From this time the sore assumed a healthy appearance, the size of the joint diminished, it became stiff, and at the expiration of three weeks anchylosis was perfect. During this period, two sinuses, one running up under the integuments of the arm, the other under those of the fore-arm, required free opening, but these incisions soon united perfectly.

Mr. Gay has adopted this mode of treating diseased joints (viz., when the cartilages are ulcerating) for many years past; in two cases of disease of the elbow-joint, similar to the above related, this peculiar treatment was, some time ago, followed, with similarly successful results; and we noticed a patient, affected with disease of the wrist-joint, who was admitted about the same time as the one whose case we have just reported, with whom the joint was opened by an incision over the posterior and inferior end of the ulna. Here, again, a large quantity of pus was discharged, and no unfavourable symptoms followed; the hand was laid on a splint, and after one month the discharge ceased, the wound healed, and anchylosis by soft fibrous tissue took place. The man, who had entirely lost the use of his hand, was now regaining much power over it.

It should be mentioned, however, that in a case of disease of the joint of the great toe, between the metatarsal bone and the inner cuneiform, the results were not favourable; but in another case, of disease in the tarsal-joint, the termination was more successful. This last patient died afterwards of phthisis, but a post-mortem examination was unfortunately not permitted.

Mr. Gay has great confidence in the success of this plan of treatment in chronic diseases of the joints—in those cases, namely, where destruction of the articulation has taken place, and nothing can be obtained (supposing amputation be not performed) but a stiff joint. In support of this opinion, Mr. Gay considers, *in the first place,* that the consequences of opening *diseased* joints are not so serious as are usually represented, and are, on the contrary, so slight as hardly to deserve notice. In most of the cases where Mr. Gay carried out this practice, the patient's general health was not in the slightest degree affected by the operation. In fact, diseased tissues, as previously stated, bear operative proceedings with less local or constitutional disturbance than is the case when sound parts are interfered with. A wound of a healthy joint may be followed by serious consequences, but it is not so with a diseased articulation.

Mr. Gay thinks, *in the second place,* that the process by which nature cures a joint in which the cartilages are removed by disease is anchylosis, but this eventual result demands the absence of cartilage. This object appears to be insured by the suppurative action which goes on in the interior of the joint, as well as the ulcerative processes which occur in the soft tissues external to the same. It is to the tardy manner in which such a process takes place that the constitutional symptoms are to be traced, for the pain which accompanies the ulceration of the capsule and external soft parts of the joint is generally very great, prolonged, and destructive of rest and appetite. No better example of the suffering consequent on matter pent up in a joint can be had than that of such an occurrence in the hip, as well as of the relief which an outlet to the matter affords. Mr. Gay, therefore, believes that laying open the joint as soon as an indication of a shedding of the cartilages is afforded allows the process of anchylosis to go on with greater rapidity, and with less detriment to the general health. The *débris* of the cartilage comes away with the more abundant suppuration which is thereby established; the restorative process follows, and is performed more speedily than in cases where the joint is left to itself.

Mr. Gay is, *in the third place,* of opinion, that the articulation is placed in the condition of a sinus which has been slit up. It is difficult to explain why a sinus having a single small aperture, and no diseased tissue in connexion with it, should refuse to heal; but such is the fact. Lay it fairly open, however, and the surface which has been pouring out tenacious serum, and appears covered with unhealthy granulations, which look as though the cells were distended with serum, assumes a healthy action, and cicatrizes. So it is with a diseased joint. Presuming that the cartilages have been shed, it may be said, that the joint refuses to assume the processes necessary to reparation, (as in the case of the sinus), until, by laying it fairly open, healthy action is established, and the joint thereby fixed, by the production of new ossific and uniting deposit. Such are Mr. Gay's arguments for opening joints when the cartilages are supposed to be about being shed, or that event has already taken place. The after treatment consists in fixing the joint in the most useful position, and keeping it steady by bandages, &c.—*Lancet, August* 24, 1850, *p.* 245.

63.—*On Iodine Injections in Hydrarthrosis.* By M. VELPEAU.—When recently dismissing two patients in whom he had successfully used iodine injections, M. Velpeau took a general review of the present state of the question of the propriety of employing them in dropsical joints, his observations forming a sort of Appendix to the celebrated Academic discussion, a few years since.

He observed, that the ill-success recorded by Boyer and other surgeons, as resulting from throwing injections into joints, much alarmed practitioners; the mischief which resulted, however, really depending upon the nature of the cases which were selected and the procedure adopted. M. Velpeau appealed against the abandonment of the practice on these grounds. For fifteen years he had employed iodine as an injection in hydrocele, without any dangerous inflammation being excited, so that he was encouraged to extend its application to various serous cysts; and in his 'Operative Medicine,' published in 1839, he suggested that it might without danger be thrown into the peritoneal cavity, an opinion afterwards confirmed by its injection into congenital hydrocele and hernial sacs. It was only in 1840, however, that he ventured to inject the knee-joint in two instances, and that by accident, the tumour in the ham communicating with the cavity of the joint. The good success of these cases, and of others in which joints were purposely injected by M. Bonnet, of Lyons, led him, however, to adopt the practice in numerous other cases. At least fifty such, treated by different surgeons, are now on record, in none of which were any alarming symptoms developed. The dangerous and fatal consequences resulted, in Boyer's cases, from the joint being widely laid open, and irritating fluids several times thrown in; while in the modern operation the smallest possible trocar is employed, and, in the majority of cases, a single injection suffices. The pain and febrile action in some cases have been considerable, requiring for their removal, in the opinion of some surgeons, leeching and abstinence; but M. Velpeau has always found them, as in the case of their appearance in hydrocele, if let alone, gradually disappear. Their occurrence at all is, indeed, rare. In general, a little redness, pain, and swelling are observed after the injection, and are soon followed by slight fever. Increasing during three or four days, these symptoms then become stationary; afterwards to diminish, and to entirely disappear about the fifteenth day, together with a part, or sometimes all the tumefaction.

M. Velpeau now considers there is not more danger in injecting a joint than the tunica vaginalis, the chief fear being, indeed, that the operation may fail. Injection, in fact, succeeds best in proportion as the cavity more nearly approaches a simple serous one. Thus, in cysts of the neck, where the membrane containing the fluid is everywhere surrounded by soft tissues, the operation never fails. In the tunica vaginalis, where the testis places the serous membrane in a less favourable condition, failures are sometimes, though very rarely, met with; while in the joints, in which the serous membrane is free only to a limited extent, and chiefly lies on solid or very hard parts, success is more rare. Still, as the employment of injection is not preventive of other means, is not dangerous, and scarcely more painful than a blister, we should always resort to it, when a simple collection of fluid resists ordinary treatment.

As, however, such collections are usually capable of being dispersed, it is an operation not often demanded, and it should not be resorted to when the collection of fluid is dependent on caries, necrosis, &c. The fears entertained by some that anchylosis might result, are unfounded, the patient recovering the use of the part as soon as the swelling has disappeared.—*Gazette des Hôpitaux.*

The French practitioners do not seem to have taken much notice of the writings of Dr. Borelli, of Turin, upon the employment of iodine injections in various affections, although he has been one of their most active advocates. In the last paper we have seen, he reiterates his opinion of their great utility, and furnishes additional cases. These consist of examples of *hydrocele* (he states, that hydrocele in boys and hydrocele of the cord in adults are always curable by simple puncture), *encysted tumours* after the evacuation of their contents, various forms of *acute* and *chronic abscess,* and *adenitis.* He objects to the use of resolvents, as being both tedious and useless. When matter is once formed, the abscess is rapidly cured by the injection. He relates, too, a case of *ranula* yielding to the employment of the injection, although simple puncture had repeatedly failed. Bouchacourt had, however, already published a similar case in the ‘ Bull. de Thérap.’ for 1843. After quoting a case of *fistula in ano* successfully treated by Van Camp at Angers, and relating one of his own, he states he has always hitherto failed in treating *fistulæ* in connexion with diseased bone; but quotes cases from the veterinary practice of Professor Peroscino, in which the iodine was successfully used in fistula connected with diseased cartilage.

Dr. Borelli considers his practice in treating abscesses by this means as somewhat peculiar, inasmuch as he seeks to obtain union by the first or the second intention, according to the requirements of the case. The suppurative process being well determined, and the acute inflammation of surrounding parts repressed by cataplasms, after opening the abscess in its most dependent part, and evacuating the contents as far as possible, he introduces a small syringe through the aperture, and throws in pure tincture of iodine with some force, allowing it to remain in, when the pain is not too great, about half a minute. He waits three or four days to see whether the plastic effusion into the pyogenic cavity will effect its occlusion. This, however, is seldom the case, unless the abscess is very small and the engorgement of surrounding tissues slight. The injection, therefore, will require repetition every two or three days, according to the amount of reaction produced; and when this is in excess, emollient cataplasms are required. The author has never seen any ill effect, local or general, following the employment of the iodine—*Omedei Annali,* Vol. CXXVIII.

The question of injecting the abdomen with iodine in *ascites* is no longer one of mere hypothesis. M. Dieulafoy resorted to it three times in the same subject, with the effect of producing a cure, and Dr. Leriche, of Lyons, has recently published two cases, in which a single injection after the complete evacuation of the fluid sufficed. M. Boinet also has recently presented a memoir to the Sociétié de Chirurgie, in which he has collected eighteen cases of ascites from various sources, wherein different substances, as gases, water, iodine, &c., have been injected. In fifteen of these success followed, and only in one did the patient die, iodine seeming to be the especially preferable substance. M. Morel, reporting upon this paper, pronounced an almost unqualified opinion in favour of the practice; but MM. Vidal, Gosselin, Robert, and others protested against drawing any such hasty conclusions from cases the history of which had been imperfectly given, and have to be confronted with others in which a fatal termination has resulted, an example of which has recently occurred in Paris.—*L'Union Médicale.*—*Brit. and For. Medico-Chirurg. Review, July,* 1850, *p.* 270.

64.—*Excision of the Knee-Joint.* Under the care of Mr. FERGUSSON, at King's College Hospital.—[Although the result of this case has not been favourable, it is brought forwards to increase the number of facts with regard to excision of the joints, as the operation has been performed successfully three times before in this country and Ireland. The patient's health had been severely tried by several previous years of suffering; but it is impossible to calculate accurately the amount of strength likely to suffice for reaping the benefits of the excision. The patient in this case was a strong-made man, twenty-one years of age, a labourer in the country, and generally enjoying good health. Three years previous to admission, he had a fall upon the knee. Violent inflammation followed, confining him to his bed for nine months; and although by active treatment he was enabled to use his leg to some degree, yet a relapse occurred, defying all surgical treatment, and he was now recommended to submit to amputation as the only means of relief. On admission, the least motion of the limb caused intense agony in the inner side of the joint, increased at night; but, with the exception of debility, the condition of the patient seemed favourable for the operation.]

Mr. Fergusson gave it as his opinion, that the cartilages of the joint were ulcerated, and ordered the limb to be put into a gutta-percha splint. As, however, no improvement took place, and the patient was anxious that active means should be employed to give him relief, Mr. Fergusson, seeing no hopes of effecting a permanent cure within a reasonable period of time, advised that, instead of amputation, excision of the articular surfaces should be resorted to, as the case seemed to him, in most respects, a favourable one for this procedure.

The patient was brought into the theatre, July 20th, and placed under the influence of chloroform, when Mr. Fergusson began the operation by making an H incision in front of the joint, the transverse line running a little below the patella. The extensor tendon was then dissected upwards, and raised with the patella like a lid, by which means the articular ends, entering into the formation of the joint, were brought into view. The ligaments were then divided, the soft parts detached from round the bones, one inch and three-quarters of the lower end of the femur, as well as the head of the tibia for about three-quarters of an inch, were sawn off from before backwards, and the sharp margins of the bones pared down with the forceps. Mr. Fergusson then cut out the patella from its ligamentous attachment; several vessels were secured, and the lines of incision brought together by points of suture. Water-dressing and a roller were subsequently applied round the joint, and the patient removed to bed, where the leg was kept extended with a pillow under the ham.

Mr. Fergusson remarked that the patient had experienced so much suffering and annoyance from the affection of the knee, that he had come into the hospital to undergo amputation. But Mr. Fergusson had long thought that excision of the knee-joint had not received sufficient attention in this country, and had resolved to put this method into practice when a case calculated for such an operation should present itself. He considered that it was cruel to remove a healthy leg and foot, as was seen in this instance, when means might be employed to save these parts. That the disease of the joint was sufficient to warrant surgical interference was now clear, for there was no vestige of cartilage left. The operation that had just been performed had been undertaken six times in these countries: twice at Liverpool by Mr. Park, twice in Dublin by Sir Philip Crampton, and twice also in Edinburgh by Mr. Syme. Out of these six, three had been successful, and in two of those which had been unfavourable, the failure did not immediately owe its origin to the operation itself. Mr. Fergusson, therefore, thought that we had not sufficient reason for abandoning the operation, and that the condemnation of some surgeons who had not much experience in excision of bones should not weigh against its revival; he had been anxious to bring the operation again before the profession. His mode of procedure had been exactly the same as that followed by Moreau and Sir Philip Crampton.

A little bleeding occurred in the afternoon, which was arrested by cold water and ice, and the limb was then placed into a fracture-box with hinged sides and a foot-board; a cushion being placed under the upper end of the tibia to keep it in apposition with the end of the femur, and water-dressing with the many-tailed bandage, were applied. Pulse quick and feeble. Morphia three-quarters of a grain.

On the third day after the operation the patient had severe rigors, much fever, and complained of pain in the head; the transverse incision had united by first intention, and the sutures of the internal were taken out to favour the escape of the discharge. On the fifth day the wound looked languid, and did not present any granulations; the end of the tibia might be seen in the wound, of an ash colour, and when the leg was raised the ends of the bones were heard grating against each other. The patient continued to have severe rigors, for which he was administered full doses of sulphuric ether, and the wound, the discharge from which was of a sanious character, was dressed and washed out with myrrh lotion. On the seventh day a very favourable turn took place; the rigors ceased; the countenance was less flushed and anxious; the pulse, which had been quick, feeble, and compressible, improved; the patient passed a good night, his appetite returned, and he expressed himself as feeling very comfortable. The wound looked much more healthy; granulations were springing up on both sides, and the discharge assumed a more healthy character. The only unfavourable symptom was a threatening of diarrhœa, for which decoction of logwood was prescribed. This state of things did not, however, last long. On the eighth day rigors came on again; the patient was bathed in cold perspiration; blood was found in clots beside the wound, but the hemorrhage was easily stopped by the application of ice. Delirium came on, with frequent loose and black alvine evacuations, and the patient sunk on the ninth day after the operation.

On a post-mortem examination all the viscera were found perfectly healthy; the veins were traced up to the heart and were quite sound. The periosteum of the end of the femur, could be stripped from the bone with great ease, and the bone itself was found, for the length of three inches, in a state of acute necrosis; the cancellated structure being full of pus, and the tibia for the distance of nearly two inches was in the same condition. Nothing was found in the intestines to account for the diarrhœa, excepting a small vascular patch in the colon stained with logwood.—*Lancet, August* 10, 1850, *p*. 183.

65.—*On Fracture of the Neck of the Thigh Bone.* By JOHN LIZARS, Esq., Edinburgh.—[Mr. Lizars, believing "the diagnosis of the fracture of the neck of the thigh bone is little understood even by surgeons holding important situations," mentions the following cases as reasons for his belief. He says:]

In the majority of cases the diagnosis is simple:—The shortening of the limb, the eversion of the toes, *the flattening of the nates*, and the pain at the joint, especially when the thigh bone is rotated, at once indicate the nature of the accident, which generally occurs after fifty years of age. But in a very considerable number of individuals, there is no shortening of the limb, for one or more days, nay weeks, or even months, after the occurrence of the accident; and, in such cases, when the fracture has been overlooked or neglected, the shortening becomes so great, that inversion, instead of eversion of the foot and toes, takes place, and the ignorant and inexperienced surgeon pronounces the case to be dislocation of the head of the thigh bone on the dorsum of the os ilium, and the unfortunate sufferer is tortured with lacs and pulleys.

The first deceptive case of this description which I witnessed occurred many years ago, under the care of the late John Bell. The case made a strong impression on my mind, in consequence of having had soon afterwards an opportunity of ascertaining the precise nature of the injury by a *post-mortem* examination. The preparation I deposited in my museum, and it illustrates fracture of the neck of the thigh bone within the fibrinous capsule, without union. Since that time, I have seen several patients under similar disfiguration of the limb, consequent on fracture of the neck of the thigh bone.

A well-marked case occurred within these four years. A gentleman, about forty-five years of age, while travelling, fell from a height of three or four feet, and lighted on his hip. He was seen by an intelligent surgeon an hour after the accident, who examined the hip-joint with great care, but could detect no deviation from the normal condition. The injured limb, after the most accurate measurement, was no shorter than the sound one, and when placed straight by its fellow, which could be easily done, there was no turning in of the toes. Six weeks after the accident had occurred, the patient could sit on a sofa, and walk, with the aid of crutches, about his room; and, while doing so, he could place the injured limb alongside the other, without any shortening or inversion of the toes, and when in this position, he could raise his leg and thigh to a considerable height, with ease and freedom.

In a few days after this the limb became disfigured, when, I understand, the patient consulted an hospital surgeon, who considered the injury to be dislocation of the head of the thigh bone on the dorsum of the os ilium; and, if he really thought so, he must have tortured the ill-fated gentleman with lacs and pulleys; but for what length of time I know not.

I was consulted in this case, within a few days subsequent to the failure of the means resorted to, and, after inquiring into the history of the accident, and carefully examining the hip-joint and limb, I stated to the patient, that if his account was correct, I suspected that he had sustained fracture of the neck of the thigh bone. The appearances of the limb were as follows:—great shortening of the limb, with marked retraction of the heel, the toes crossing the sound foot over the tarsus; *flattening of the hip joint;* extensive mobility of the limb, even through the efforts alone of the patient, and with marked abduction. Without any assistance he could perform flexion, extension, and abduction, in a free manner, and to a full extent.

A case, parallel in many respects to the last, presented itself to me about two years ago:—A lady, about fifty-five years of age, in attempting to descend from the step of the door of a first-class railway carriage, fell down. When carried home, her ordinary medical attendant, a very superior surgeon, was called, and as he could see nothing commensurate with the pain and lameness, he sent for a surgeon of great reputation, attached to one of the largest hospitals in a metropolitan city. After the most careful examination they could discover no fracture, and were satisfied at the end of three months' attendance on the case, that none existed, at which period the lady set out for Edinburgh. On her arrival here I was consulted, and found unequivocal marks of fracture of the neck of the thigh bone: the limb being shorter, the toes everted, and *the hip flattened.* The reason for the toes in this case not being inverted was, I presume, that the periosteum and fibrous capsule had not been torn so as to allow such a separation of the head of the thigh bone from its shaft, as occurred in the case referred to of Mr. John Bell, and others yet to be detailed.

When I was officiating as surgeon of the infirmary in this city, my clerk or house-surgeon, Dr. William Home, one of the most talented surgeons of the day, now staff-surgeon in the army, remarked to me one day (after I had made some clinical observations to the students, respecting a man whom I had dismissed cured of fracture of the neck of the thigh bone, with little or no shortening of the limb), that he was doubtful if fracture had existed in some of the cases I had treated as such, and that he would esteem it a favour, if I would allow the next patient who came in, with supposed fracture of the neck of this bone, to lie in bed without a splint. To this proposal I assented. Not many days elapsed before a man was admitted into the hospital with injury of the hip-joint, which Dr. Home considered merely a bruise, while I pronounced it fracture of the neck of the os femoris. The man was about fifty five years of age,—had fallen from a height of not more than 3 ft., and lighted on his hip. There was no shortening of the limb, and no eversion of the toes, but slight *flattening of the natis,*—an appearance which I consider, along with other symptoms, strongly characterises the nature of this accident. He felt little or no pain in moving the limb, and there was no crepitus. The man was laid in bed on his back without any splint, and the joint fomented. The limb was examined daily by Dr. Holme and myself, and on the 8th day we perceived a slight shortening, with the heel more drawn up than the toes. Dr. Home was then convinced that fracture of the neck of this bone existed, and applied the long splint of Desault. In this patient a tolerable cure was effected.

I have met with three cases, where this fracture simulated morbus coxarius, viz., flattened natis, sinuses extending down the thigh, shortened limb, with inversion of the foot and toes. On dissection the head of the os femoris was in a state of necrosis, and far apart from the neck and trochanters. The patients were upwards of fifty years of age:—in one, the fracture was overlooked; in another, it had been mistaken for a bruise; and in the third, for dislocation on the dorsum of the os ilium; in which last, the patient had been tortured with lacs and pulleys by an hospital surgeon.

In Part VI. of my Anatomical Plates, 1st edition, p. 157 of the descriptive letter-press, I pointed out to the Profession, that fracture of the neck of the thigh bone is liable to be mistaken for dislocation of the head of the os femoris on the dorsum of the os ilium. I remarked, that "the powerful extensors drag the trochanter major upon the dorsum of the os ilium, and the gluteus medius pulls it forward, or to the pubic region, until the limb assumes the same appearance, as in luxation on the dorsum of the os ilium."

In my work on Practical Surgery, 2nd edition, p. 128, I state, "the neck of the thigh bone is often fractured, and in many there is no alteration of the limb until some days after the accident, and if neglected, the toes become ultimately inverted."

I am of opinion, that in all cases of injury of the hip-joint, if there is *flattening of the natis,* and especially if the individual be upwards of forty years of age, we ought to look for fracture of the neck of the thigh bone, and apply Desault's long wooden splint, and retain it for six or eight weeks, if the skin over the sacrum remains sound, and the health of the patient will bear it. I deposited in my museum a preparation of ossific union within the fibrous capsule, which occurred in a man upwards of sixty years of age, and I am confident that I have succeeded in uniting this fracture in several patients. But, however strong and neat the junction may be, still the length of the limb will be reduced to a trifling degree; the shortening proceeding unavoidably from interstitial absorption, and in consequence of the angle being changed between the shaft and neck of the bone, from the obtuse to the right, or even the acute angle.—*Med. Times, July* 6, 1850, *p.* 8.

66.—*Case of Ununited Fracture of the Tibia, of Twenty-four Years' Standing, successfully treated.* By R. W. TAMPLIN, Esq., Surgeon to the Royal Orthopœdic Hospital.—[Miss —, æt. 25, slipped, at the age of 14 months, between the bars of a garden-seat. A little discolouration followed at the lower third of the leg, and a slight curvature of the bone. On the child walking less strongly, a surgeon was consulted, who consoled the parents, "that all would be well in a few months." Another surgeon being called in, discovered the fracture, and pronounced it "falsely united." The case under treatment now appeared to be progressing satisfactorily, when the child happened another accident, and the smaller bone of the leg is stated to have been broken. All means which were tried to procure union of the fracture completely failed, and the general health became greatly impaired. The case was then pronounced incurable, and amputation was the general advice given to the patient. Although an instrument was contrived by which she managed to get about, yet it was not without great pain, the weakness increasing in spite of the artificial support.]

On the 17th of July, 1849, I first saw the patient, and, upon examination, found that the leg had been fractured at the lower third obliquely upwards, the upper portion of the tibia projecting pointedly forwards and riding over the lower third. The leg was two inches and a half shorter than the other. It was freely moveable. The knee, from the constant irregular strain upon the ligaments, yielded outwardly, and the patient could not put the leg to the ground, even with the assistance of the support, without suffering severe pain both at the point of fracture and in the knee-joint. Her general health had suffered more or less, and her existence is stated to have been a burthen to her.

From the history of the case,—the fact that it had existed from childhood, that all attempts had failed, that amputation was the general advice given,—I gave a doubtful opinion, but determined to make an attempt to obtain a union without operation. Three indications occurred to me: first, to bring the bones in apposition; the second to obtain absorption of the false uniting medium; the third, to endeavour to obtain union, either by the deposition of bony matter, which I thought might possibly be thrown out from the irritation which must necessarily be set up, or by a contraction of the portion of the false union which immediately surrounded the fracture. With these views I ordered an instrument to be so made that the thigh could be firmly grasped above the condyles of the femur, the foot below having a screw by means of which the distance between the knee and foot could be gradually increased. This instrument was applied on the 31st of July, 1849, and the leg kept horizontal, in the extended position. Gradual extension was now commenced, and continued for four days. So much pain was occasioned in the gastrocnemius, that I found it necessary to divide the tendo-achillis, which was done in the usual manner by puncture from within outwards. The instrument was omitted for eight days, and then re-applied, and extension again used. From this time the leg became gradually and easily elongated; and during the extension a steady continued pressure was kept up on the tibia above the point of fracture, and counter-pressure at the back of the leg, just above the ankle-joint. This treatment was persevered in unremittingly until the 9th of January, 1850, without interruption to the health; there was, however, at times severe pain, which was allayed by opiates, when, upon examination, the leg was found to be of equal length with its fellow, and the bone retained its position unassisted. I then applied the common splints, and ordered an instrument, with a boot attached, to support the weakened knee-joint, which had resumed its proper relative position during the treatment, and also having a pad to support the tibia, in case the union was imperfect.

On the 26th of February, the patient could raise the leg in the horizontal position without the slightest pain, and without a sign of motion at the point of fracture. I then applied the new support, which was worn, without any attempt to put the foot to the ground, until the 8th of March. She was able to stand on the 3rd of April; and then, at the request of Mr. Travers, and afterwards in the presence of Mr. Lawrence, both of whom kindly visited the patient, she walked without pain, and without a sign of motion at the point of fracture. Since that time she has continued to use the leg freely, with slight intermissions from indisposition. Can walk up and down stairs. Her general health has improved, and I have every reason to believe that a perfect cure has been effected.—*Med. Gazette, July* 26, 1850, *p.* 140.

67.—ON FRACTURES OF THE RIBS.

Reported by Dr. Charles Kidd, Guy's Hospital.

[Fractures of the ribs are now treated in this hospital regularly by long strips of plaster being applied over one side of the chest. The first case illustrating this mode of treatment, was in that of Thomas Browne, who happened an accident by being precipitated off a plank on the edge of his coal barge. He complained, on admission, of the ordinary symptoms, with intense pain in the side, increased on inspiration, and the fifth, sixth, and seventh ribs were found broken.]

The side was strapped by passing large strips of adhesive plaster round the top of the chest, down over the site of the injury to about midway between the last rib and crest of the ilium; he was ordered a mixture, with hyosciamus and tartrate of antimony, and strict rest (not a word to be spoken) enjoined. This circumstance Mr. Hilton dwells on as essentially necessary in these cases;—indeed, with absolute quiet, the strapping carefully managed, and the tartrate of antimony sedulously attended to — especially if perspiration break out — the rapidity with which these cases do well is quite wonderful.

29th.—The patient to day is much the same; symptoms not much relieved. Ordered to continue the antimony and hyosciamus, and to go on exactly as before.

June 1st to 7th.—Went through all the familiar changes of this accident, and was so well on the 7th as to discontinue his medicines; in fact, he is quite well to day.

10th.—Chest strapped again to day. Left the hospital.

Thus, in, we may say, twelve days from the accident, this man leaves the hospital. It would be expecting too much, however, to think that perfect bony union had been established. The ribs, it need scarcely be said, are very vascular; the reparative power proportionally active; the man, therefore, having been cautioned what to do was discharged. He is now perfectly well, and perfect bony union all but completed.

Case 2.—M. A. H. This case is more serious. The woman was in the habit of drinking; and in a domestic feud was beaten by her husband with a poker. On applying at the hospital she appeared in a most miserable state; very sick, with pain in the side; cough and usual signs of fractured ribs. On examination, the fifth, sixth, and seventh ribs, as in the previous case, were all found broken, with very extensive emphysema all up the side. The strips of plaster were carried from behind forward, over the chest, as before, the mixture, with tartrate of antimony and hyosciamus also exhibited systematically, and strict rest enjoined.

26th.—Feels much better to day; quite different, in fact, since the plaster was put on.

27th.—Still improving; emphysema manifestly disappearing; pulse 84.

28th.—Same report.

29th.—Left the hospital to day quite another person.

Case 3.— — Lewis, aged 47, is a case of fractured rib with laceration of the lung, bringing on pneumonia. This man, standing with a crowd at Epsom, at the races, was precipitated a distance of sixteen feet to the ground. On being taken up he was found to have broken the eighth and ninth ribs; crepitation of the two ends of the bone was quite apparent, as well as the other signs of fracture. He was put under treatment much in the same manner as in the two former cases; straps of plaster, 2½ inches broad, were put round the chest, and antimony ordered.

13th.—He was hot and very feverish; restless. To continue the antimony.

14th.—Worse to day. Evident symptoms of pneumonia; pain in chest; rusty-coloured sputa. He was ordered calomel, in addition to his other medicines.

16th.—The report of the last two days more unfavourable. He is exceedingly ill to day; quite delirious. The calomel had opened the bowels; his voice is whispering and weak; expectoration still rust-coloured; pulse 90; evidently in great danger.

19th.—Better; pneumonia subdued.

24th. —So much improved as to sit up. Was ordered to day cascarilla and tinct. cinchonæ, with additional nourishment.

June 13th —Has been improving steadily up to this date. Straps renewed over the chest, and he left hospital.—*Medical Times, July* 6, 1850, *p.* 11.

68.—ON A NEW SWINGING APPARATUS FOR THE TREATMENT OF FRACTURE OF THE LEG.

By S. Jas. A. Salter, Esq., M.B., late House-Surgeon to King's College Hospital, &c.

(Read before the King's College Medical Society.)

[Mr. Salter brings this subject before the profession, because he believes it has some important advantages over those apparatus already in use, and because the plan of swinging broken legs in their treatment is attended by great benefit and immense comfort to the patient. He says:]

In the various swings I am about to mention, other attempts have been made besides swinging the leg, and allowing the limb, as well as the body, a certain amount of motion. Various splints and extensive apparatus have formed essential parts of some of these combinations; and their elaborateness and complication have, in some cases, reached a most marvellous extent.

These are the different apparatus I have been able to discover in the various works I have consulted:—

I. *Prael's apparatus.*—The only principle attempted in this device is that of swinging. The apparatus is a horizontal beam attached to the ceiling, from which the leg is suspended by cords, which pass through pulleys at the extremities of the beam. There is no particular splint belonging to this arrangement. It was first published in 1801.

II. *Schmidt's apparatus* is like Prael's, with the addition of a fracture-box, supplying the place of splints. At the extremities of the fracture-box are some straps and screws, by which prolonged extension of the limb can be effected. This was first described by Schmidt in Loder's 'Journal für de Chirurgie,' 1805.

III. *Hecker's apparatus.*—Hecker's apparatus consists of an arched iron bar extending transversely across the bed over the patient's legs; from the centre of this arch depend four chains, which are attached to a fracture-box in which the leg is placed. There are two smaller arches of iron attached to the extremities of the fracture-box and arching over the foot and knee, which serve the purpose of an ordinary cradle to keep off the bed-clothes. The bed-clothes cover over the whole of this apparatus, in this respect differing from those previously described. The fracture-box is furnished with an extension arrangement similar to Schmidt's. This apparatus was described and figured in Gräfe's 'Journal der Chirurgie' in 1823.

IV. *Nusbaumer's apparatus.*—This is simply a modification of Prael's, in which the inventor endeavoured to accomplish an alternate lateral posture, as well as the swing. It is a very complicated impracticable affair. First described in 1825.

V. *Dornsbluth's apparatus.*—This differs from Prael's no further than this, that the horizontal beam, from which the leg is suspended, is connected to the ceiling by an elastic coil of wire, instead of an inelastic cord, as in Prael's. The coil of wire is stiff and elastic, one end is attached to the ceiling and the the other to the centre of the beam, and it is enclosed in a box for protection. Published in 1829.

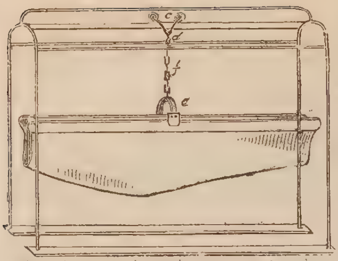
VI. *Mr. Grantham's apparatus* is a contrivance consisting of a rest in form not unlike a Macintyre's splint, having a thigh-piece, a leg-piece, and a foot-piece. On this the leg is placed (after splints have been applied) and strapped on. It is then swung by two ropes, one passing under the rest at the ham, and the other under the ankle. These ropes are attached to "anything which may be contrived at the top of the bed or over a couch." Published 1833.

VII. *Mr. Luke's plan* consists "merely of a leg-rest, two splints, and a cradle." The "leg-rest may be made either of wood or sheet-iron, the upper surface of which is hollow and roughly shaped to the form of the back of the leg. The rest is of sufficient length to reach from the foot to about five inches above the knee on the posterior surface of the thigh, and so fitted to the ham, as to afford support to the extremities both of the tibia and femur. The two (side) splints and the cradle being similar to those in ordinary use, need not any description. In applying the apparatus to its proper use, the leg-rest and splints being sufficiently padded, the fractured limb is adjusted on the former and secured by the latter, placed one on each side, and fastened by tapes carried around the whole in the usual way. This being accomplished, the rest, &c., is suspended by means of two other tapes, passed under it and over the middle bar of the cradle, now put in a convenient position over the limb. One should be placed near the ankle-joint, and the other a little below the knee." This is a simple and efficacious plan, and one very easily applied. It was first described in 1841.

VIII. *Torbock's apparatus.*—This device consists of a very strong iron vertical rod, which is fixed to the side of the bed by a clamp screw; this screw is capable of being elongated by rack-work, which is worked by a key. From the summit of this vertical rod a horizontal one stretches across the bed, attached by one end to the vertical rod, and free at the other. The horizontal rod rotates upon the vertical, so that its attached end is the centre of a circle of which *it* is a *radius*, and the line described by the movement of its free end an *arc*. Its form is like that of a sign post of a country inn. The horizontal beam is a polished rail, upon which two wheels run (adding thus a second motion to the apparatus,) and these are connected to a crib below, in which the leg is placed, similar to the one used in my apparatus. The crib is underneath the bed-clothes, and the iron frame-work external, so that they are connected by an intervening portion, the motions of which are almost wholly prevented by the contact of the bed-clothes.

IX. I now have to ask your attention while I give you a detailed account of my own apparatus, (of which a diagram is given on the next page). It consists of a cradle, similar in form, but rather larger than the one ordinarily used for fractures of the leg; and it performs the office both of a protecting cradle and of a swing, thus far resembling Mr. Luke's, but no farther.

There are two iron arches, connected by six horizontal rods; the rods connecting the extremities of the arches are flat and heavy, so as to make the cradle rest firmly upon the bed. The lower of the two rods connecting the summits of the arches, is a rail of polished steel, on which run the wheels (*c*) that are placed on it, all pressure of the bed-clothes on these wheels being prevented by the rod which extends over



the rail. The *train* consists of two brass wheels, and from the axes of these, iron plates extend down on each side to a swivel hook (*d*) underneath the rail. This train readily moves up and and down the rail. From the hook hangs a chain (*f*), consisting of several links, and attached below to the summit of an arched piece of iron—the *span* (*e*). The span is a narrow strip of iron, bent in a semicircular form, having at the extremities two buttons on each side, by which it is attached to the remaining portion of the apparatus—the *crib*. The crib is composed of two flat laths of mahogany, from which hangs a sort of bag of jean for the reception of the broken rib; attached to the strips of wood are two little plates of brass, perforated so as to admit the buttons at the extremities of the span, and by this means the sides of the crib are kept sufficiently near, and also kept sufficiently apart to form a convenient receptacle for the leg.

Now, nothing can be simpler or easier than the application, removal, or readjustment of this apparatus. It is not meant as a substitute for splints, but after the application of splints the limb is placed upon the expanded crib, which is laid out on the bed underneath it, the sides of the crib are then brought up and buttoned on to the extremities of the span; the cradle now being placed over the leg, the span, and with it the leg, is raised, and its hook inserted into one of the links of the chain, that link being chosen the height of which best suits the patient. The adjustment of this contrivance is the work of a moment. It is a matter of indifference what splint is used, for the swinging is quite independent of the setting up of the leg, and adjustment of splints, and is superadded to it. A single or double side-splint, with a foot-piece, may be employed; or, what I prefer, the lower part of a Macintyre's splint, with the thigh-piece, extension screw, and foot-rest removed. This receives the leg and foot very comfortably and firmly, and they may be bandaged up in the ordinary way, and independent of the swinging apparatus, which is applied afterwards.

The mechanical advantages, which it was my object to gain by the apparatus I have now described, are partly common to other forms, and partly peculiar to itself, and are principally these:—Complete protection from the bed-clothes, so that they shall not interfere with the motions of the leg. In all the other apparatus I have mentioned, excepting Luke's and Hecker's, one extremity of the cord, by which the limb is swung, is external to the bed-clothes while the other is within. The effect is, that in passing into the bed almost all motion is prevented by the entanglement of the blankets and the arrangements which are necessary to keep the clothes from pressing on the foot and leg, so that the object of the contrivance is pretty nearly neutralized thereby. I may enumerate the facility of application and simplicity, and the graduation in height of suspension, as advantages which my apparatus possesses.

The running of the train upon the rail which supports the leg, allows a very considerable amount of motion without disturbing the relation of the parts; the leg may be moved *en masse* eighteen inches without altering the relative height of the heel or knee, so that the patient may completely alter his position in bed without disturbing the adjustment of the leg. From this circumstance, when the leg is thus swung, the patient can sit up in bed to take his meals, or to read or write; and I must here observe, that when an invalid sits up in bed, that act does not consist in merely assuming the sitting posture—in bending the trunk upon the thighs—but he always instinctively, as it were, draws himself up towards the head of the bed a foot or so, so as to rest his loins against the pillow and bolster, from which he receives no small amount of support. This you may easily see for yourselves, by telling the next patient you visit to sit up in bed; you will see how he involuntarily draws himself up, and rests his back against the pillow, so as to place his buttocks where his shoulders were previously, when he was lying down just below the pillow. Now the motion of the train upon the rail, and with it the crib and leg, just allows this posture to be assumed at pleasure by the patient. In the other swings placed under the bed-clothes (Hecker's and Luke's), this is prevented, and the amount of movement very limited, from the fact that the upper attachment of the suspending cord or chain is fixed, and the only motion that the limb enjoys is the swinging in a small arc of a circle, of which the suspending cord is a radius, and its upper attachment the centre.

From the action of the swivel hook the patient gets another movement —he can shift his body from one side of the bed to the other, while the foot moves in the opposite direction, the leg moving in the same plane upon the axis of the swivel. I cannot understand that this movement is allowed in Mr. Luke's arrangement, at least to any appreciable extent, as the tapes by which the leg is swung are at both extremities of the leg, and would seem to me to counteract such a motion; whereas if the tape were single, and attached to the centre of the leg, the twisting of this tape would be equivalent to the rotation of the swivel.

I need scarcely point out to you the great advantages which arise from any apparatus or arrangement which allows a patient, confined to bed, some change of posture—some relief or change of pressure from those parts of the body on which he rests when lying down. Any one who has seen the most moderate amount of hospital practice, must have observed the distressing consequences which are entailed upon those patients who, through a protracted illness, are confined to bed in one posture, without any change of position. The accumulation of filth and moisture under the patient, the inconvenience and difficulty with which he passes his evacuations, the obstacles to changing his bed linen, and, above all, the horrid bed-sores are to be attributed to this circumstance. Bed-sores are pre-eminently the destruction of the bed-ridden, and result from protracted pressure upon some unrelieved projecting spot—the sacrum, the hips, the scapulæ; and whether it results from paralysis, where the patient does not feel pain in the part pressed upon, and therefore does not care to move his position, or where, from loss of motor power, he cannot change posture if he did feel it; or whether it results from the patient being tied down to his bed by a fixed immovable splint, the result is the same—the skin of his back becomes gangrenous from pressure, a slough comes away, and an unmanageable sore is left, only to be enlarged by the still existing cause. How common is it for a patient who has to pass through the tedious weeks or months which are required to cure a *compound fracture* of the leg, especially if his case be complicated, as too often happens, with *necrosis, exfoliation, abscess*, or even, *erysipelas*—his leg being fixed all the while in an immovable splint —to sustain bed-sores; but, swing his leg, put him in circumstances where he can sit up when he likes, or lie down when he likes, so that he can pass his evacuations with cleanliness, and be kept dry, and relieve the pressure on any particular spot, and then you will find the cause of the bed-sores removed.—*Provin. Med. and Surg. Journal, Oct.* 16, 1850, *p.* 564.

69.—NEW PROCESS FOR THE REDUCTION OF THE JAW.

By M. Nelaton.

According to M. Nelaton, the obstacle to reduction existing neither in the resistance offered to the condyle by the eminence of the transverse apophysis, nor in muscular action, it must be sought for, not in the articulation itself, but in neighbouring parts.

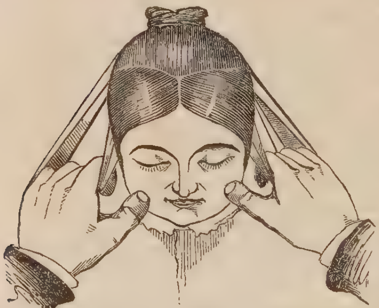
"In front of the tempero-maxillary articulation," says M. Nélaton, "we find the tempero-zygomatic fossa, in which the coronoid process is lodged when the mouth is closed. Before and behind this excavation are placed two eminences, the posterior formed by the transverse root of the zygoma, the anterior by the articulation of the superior maxillary with the malar bone. At the inferior part of the suture which results from the union of these two bones, there exists a tubercle sufficiently prominent, limited within by a notch formed by the smooth edge of the malar process of the superior maxillary bone, and often on the outer side by a little, elongated, almost oval fossette. This eminence, to which we may give the name of malar tubercle, is situated at about the distance of a centimetre from the coronoid process. In place of this tubercle we have sometimes met a plane surface, and even in certain subjects a notch more or less deep; but the presence of the tubercle is the rule. With regard to the coronoid process this latter presents great differences; very short in some, and elevated scarcely to the level of the condyle, it is found very much elongated in others; sometimes directed upwards, at other times obliquely outwards, so that its summit tends to meet the zygomatic arch; in some instances directed forwards, and distant from the condyle; in others directed backwards, so as to approach it." "These facts well established," continues M. Nélaton, "let us examine the pathological condition. Having, as I have said, undertaken some experiments on the dead body, with a view to verify the prevailing doctrine on luxations of the lower jaw, I have ascertained:—First, as M. Malgaigne observes, that if the condyle of the lower jaw is carried forwards without passing the point which the cavity of the capsule permits it to reach, the displacement disappears forcibly as soon as we approximate the dental arches, the eminence of the transverse apophysis presenting no obstacle to the return of the condyle. Second, that if the anterior part of the capsule be cut or torn, so that the condyle can pass out of it and advance a few millimetres, we remark that the displacement is permanent, not, as is generally believed, because of the elevation of the transverse root, nor by reason of the contraction or tension of the muscles, but because the summit of the coronoid process comes to butt (*arc-bouter*) against the inferior and anterior angle of the malar bone, and is lodged in the little fossette which we have said exists often at this point. The contact of the summit of the coronoid process with the malar bone appears to us, then, to constitute an indispensable condition in the true dislocation; and for this the displacement need not be extreme; it suffices that the condyle be advanced from three to four millimetres. The external lateral ligament remains intact, the capsule alone is torn at its anterior part, and the inter-articular cartilage either accompanies the condyle in its displacement, or remains beneath the transverse root, according as the rupture is either above or below its anterior edge. It results from what precedes that it is not on the condyle that we must fix attention to find the cause which renders the dislocation permanent, but on the coronoid process and the malar bone, since it is in the contact of these two bones that almost all the difficulty of reduction resides."

The following case presents a remarkable contrast between the method of reduction generally adopted, and that of M. Nélaton:

"A woman luxated both condyles of the lower jaw, whilst yawning, about 11 o'clock in the evening. M. Beaugrand, summoned on the instant, had made many attempts at reduction without success. Two other surgeons having been called in, tried several methods, and finally, at 2 o'clock at night, the patient being wearied out, was left with the luxation unreduced. On the next day the efforts at reduction were renewed, but equally without success, and on the day following (thirty-six hours after the accident) M. Nélaton was summoned. On his arrival, the patient was suffering much, the cheeks and mouth being swollen and œdematous from the pressure exercised in the attempts at reduction; and even the mucous membrane of the mouth was excoriated. The mouth was open, the dental arches separated only to the extent of a centimetre; the lower incisor teeth on a plane anterior to the upper. There was an invincible obstacle to approaching the jaws; but it was possible to open the mouth further, and the separation of the incisor teeth could be carried as far as two centimetres."

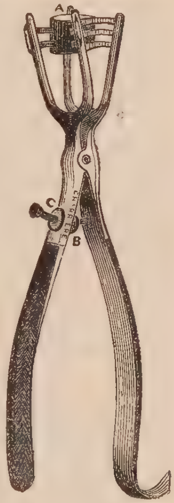
The depression in front of the meatus auditorius was well marked. M. Nélaton proceeded to examine the patient; the index finger of the right hand was introduced into the mouth, and placed on the anterior edge of the coronoid process, so that the palmar aspect of the finger, in following it up, came to the summit of the coronoid, which was then manifestly found resting in front of the malar bone. "All these points established," says M. Nélaton, I proceeded to the reduction, for which I ordered the patient to open the mouth as much as possible, and whilst she executed this movement, I placed my two thumbs on the coronoid processes, and without even embracing the jaw, without taking any other point of support, a simple pressure in a backward direction caused the condyles to return suddenly into their cavities; the reduction was complete, and all the symptoms disappeared. What theory had made me only presume was supassed by reality, and the new process was found not only equal but superior to the old, even when put in execution by skilful surgeons." Two other cases of successful application of this process are recorded by M. Nélaton. In one, reduction had been unsuccessfully attempted by pushing the coronoid process upwards and backwards, thus approximating more closely this process to the malar bone. The patient was brought to M. Nélaton, who, placing himself behind, took with his thumbs a point of support on the nape of the neck, and commanding the patient to open his mouth, exerted a slight pressure upon the coronoid processes. by means of the fingers placed externally; a slight noise was heard, and the condyles resumed their places, the countenance of the patient recovering its normal configuration, and the mouth its functions. It is but right to add, that M. Chevalier, interne of the hospital of St. Louis, and M. Vassor, who had seen M. Nélaton operate, failed in the application of his process, though they took the precaution of causing the patient to open his mouth; reduction was finally effected in this case by the ordinary method.

To succeed, according to the process of M. Nélaton, it is necessary to act either by the interior of the mouth, taking a point of support behind the mastoid processes, or externally, by the operator taking a position behind the patient, and making pressure on the coronoid process, pushing it downwards and backwards, to disengage it from contact with the malar bone, at the same time that the patient opens the mouth. In ordinary cases a light pressure is sufficient: but if more force be requisite, M. Nélaton advises the head to be supported by an assistant, or a band to be passed around it, in which the operator can engage his index and middle fingers, while the thumb must be brought to bear on the coronoid process. The accompanying figure exhibits a simple and efficacious method of employing a handkerchief to give a sufficient fulcrum.



The method of reduction which we have now described has been before the profession for more than a year.—*Dublin Quarterly Journal of Med. Science, Aug.* 1850, *p.* 109.

70.—*Reduction of Dislocated Thumb.* By M. Blandin.—The accompanying figure represents an instrument devised by M. Luër for the reduction of dislocations of the thumb. It has been successfully employed by M. Blandin. That originally invented by M. Luër has undergone some modification and improvement in the hands of M. Charriere, whose name is so well known in connexion with the manufacture of surgical apparatus. The modified instrument of M. Charriere consists, as shown by the wood cut, of a forceps, the blades of which, instead of being simply enlarged, are bifurcated. The parallel arms resulting from this bifurcation of each blade are furnished with straps of leather, A, interlacing in two-thirds of their extent, and thus forming a double knot, which is opened by the separation of the blades which form the handle. The action and power of this double knot can be limited and maintained by two screws, B, C, in such a manner that, without employing much force, a sufficiently great pressure can be exerted, which, at will, may be increased, diminished, or kept fixed. This forceps, the application of which is obvious, may be employed indifferently for all the fingers, and, indeed, on a larger scale, might be capable of more general application. With some slight modification of the handles, and the addition of rings, or other means of attaching the cords of a system of pulleys, it would be of great service in the reduction of dislocations of the limbs. The original instrument of M. Luër, though used with success by M. Blandin in the first case, completely failed in the next. It is, however, but just to add that the ordinary means of reduction had also been ineffectual, though aided by subcutaneous section of the lateral ligaments, and the attachments of the muscles of the thenar eminence. It is only, then, in recent cases that we may expect success from the employment of the instrument, which, it will be at once confessed, presents many advantages over the ordinary means, especially the rude though ingenious application of the door key, or the well-known clove hitch.—*Dub. Quarterly Jour. of Med. Science, August,* 1850, *p.* 117.



ORGANS OF CIRCULATION.

71.—ON THE EMPLOYMENT OF FORCED FLEXION FOR ARRESTING HEMORRHAGE IN WOUNDS OF THE PALMAR ARCH.

By E. DURWELL, Esq., Guebwiller (Haut Rhin).

[Mr. Durwell, in a case of wound of the palmar arch from the fragments of a broken bottle piercing the palm, found himself, on arriving at the cottage of the patient, with no means of securing the artery; and,]

While controlling the hemorrhage by pressure on the brachial artery, the following sentence in M. Malgaigne's 'Anatomie Chirurgicale' occurred to his mind:—"The only points at which obliteration of an artery can be obtained by position alone, without the aid of external compression, are at the bend of the arm and knee—a fact which is of great importance in reference to the arrest of hemorrhage." Acting upon this statement, Mr. Durwell immediately bent the arm on the forearm at an acute angle; the hemorrhage was instantly arrested. Advantage was taken of the circumstance to effect a definite cure. The arm was retained in its flexed position by bandages, so that the pulsation of the radial artery was completely intercepted. The wound of the hand was treated as an ordinary wound, and, for the sake of precaution, compresses were laid over the course of the arteries of the forearm. The cure progressed favourably. On the third day, as the patient complained of the restraint of the posture, the arm was slightly extended, and it was noticed that a small portion of florid thin blood oozed from the wound. The arm was restored to its flexed position, and in a short time the vessels and the external wound had perfectly healed.

In this manner a wound, usually regarded as of a very serious character, was safely and speedily cured by a proceeding as simple and unobjectionable as has ever been proposed. It must be attended with success when employed in analogous cases, and, from its simplicity, should be tried in the first instance in every case. Mr. Durwell observes justly, that it would be rash to make a more general application of a single fact, as there may doubtless occur complications, rendering its employment impossible or unadvisable; but, at the same time, he adds that this principle may be found, by further experiment, to be useful on other occasions, for the obliteration of the popliteal and humeral arteries. This principle he lays down in the proposition—"that in most arterial lesions of the forearm and leg, prolonged forcible flexion supersedes the ligature of the vessels."—*L'Union Médicale.*

[We find that the pulsation at the wrist is entirely stopped by firm flexure of the arm at the elbow-joint: hence hemorrhage from the ulnar and radial arteries may be thus controlled.—ED. MED. GAZETTE.]—*Medical Gazette, Aug.* 23, 1850, *p.* 351.

72.—*On the Origin of the Epigastric and Obturator Arteries by a common Trunk from the Internal Iliac; with an Inquiry into the amount of Danger occasioned by various Positions of Arteries in the ordinary Operations for Femoral and Inguinal Hernia.* By Dr. P. REDFERN, King's College, Aberdeen.—[We are sorry we cannot find more room for Dr. Redfern's learned treatise on this subject. The conclusions arrived at by this excellent physician, after his elaborate investigation of this subject, are,]

1st, In every possible variety of inguinal rupture, the stricture ought to be divided directly upwards, as by so doing there is not the slightest risk of hemorrhage.

2d, The probability of the occurrence of hemorrhage is much greater in operations for femoral than for inguinal hernia, as the epigastric, obturator, or internal circumflex artery may be divided in exposing the sac; and the obturator, epigastric, or a large anastomosing branch, may be injured in the division of the stricture.

3d, The varieties in the arteries named are sufficiently frequent to render them of great surgical importance, and that variety is most important in which the vessel is displaced inwards, by the descent of a femoral hernia, and becomes tensely stretched over the front and inner side of the neck of the sac.

4th, The risk of hemorrhage is not lessened by dividing the stricture of a femoral hernia in any other direction than inwards, or upwards and inwards, but it is very much diminished by dividing as few of the tense fibres of the edge of the ring as possible,—by avoiding a sawing motion,—and by pressing the edge of the knife rather upon the anterior surface than directly on the edge of the fibres constituting the stricture.

5th, In a male, whose testis has passed into the scrotum, the spermatic artery may be wounded, and the testicle lost, by dividing the stricture at the neck of the sac of a femoral hernia upwards, and, therefore, this method of operating ought to be relinquished. —*Monthly Journal of Med. Science, September*, 1850, *p.* 222.

ALIMENTARY CANAL.

73.—CASE OF MALIGNANT DISEASE OF THE TONGUE, IN WHICH THE LINGUAL GUSTATORY NERVE WAS DIVIDED.

By John Hilton, Esq., F.R.S., &c.

[This was a case of a female aged 39, admitted into Guy's Hospital, Feb. 26, 1850, having a large, excavated, ragged ulcer, with hard, raised, inverted, irregular edges, occupying the left side of the tongue, and of considerable extent. The patient suffered much from severe pain within the head, and from a profuse flow of tenacious saliva, with an inability to take solid food, from the pain produced in the ulcer thereby. Near the os hyoides was a gland deep in the neck, somewhat enlarged. Her pulse was about 80 in a minute. Internal viscera healthy. After observing the effects of actual cautery to the projecting and painful edges of the ulcer, and the administration of conium internally, Mr. Hilton decided on removing the tumour by excision. In consequence, however, of the greatly swollen, inflamed, and painful state of the tongue, the operation was deferred. Sixteen leeches, and a hemlock poultice were applied to the neck; pieces of ice were kept in the mouth, and ext. conii, gr. v., were ordered to be taken every six hours, and aperients occasionally. Under this treatment the swelling soon subsided. The ulcer improved in appearance, but did not diminish in size.]

The disease of the tongue was progressing; the ulcer too sensitive to permit food to be taken; the salivation distressing; and she had great pain over nearly the whole of the distribution of the fifth nerve on the left side. Believing that many of these symptoms might be relieved if the sensibility of the ulcer could be destroyed, and knowing that the destruction of the sensibility of the tongue would render the operation of sloughing off the diseased parts by the application of ligatures nearly painless, I determined, if possible, to divide the lingual gustatory branch of the fifth nerve between the disease and the brain.

20th.—I placed the patient in the recumbent position, but with the head a little raised, opposite a good light. My dresser, Mr. Morgan, drew the tongue (which was much congested with blood) forwards, upwards, and towards the patient's right side. I then divided vertically, with a small knife, the mucous membrane, and submucous tissue to about three-quarters of an inch in length, opposite the molar teeth, over the hyo-glossus muscle, and across the position of the upper margin of the sublingual gland; the progress of the operation was much impeded by the almost constant flow of blood, chiefly venous, into the wound; but, by continuing deeply by the side of the tongue, this first incision, through the upper edge of the sublingual gland, I exposed the nerve; and, laying hold of it with forceps, I divided it with scissors; immediately all sensation was lost in the anterior part of the tongue, and in the ulcer; either part could be seized and pinched with forceps without the patient being in any way aware of it. The bleeding was stopped by a piece of sponge, dipped in a strong solution of alum, being forced into the wound. After the operation the patient had a good deal of pain in the left side of the face, and in the left ear, and the soft parts in the left side of the neck became inflamed and swollen. Leeches and poppy fomentation subdued this condition in a few days. During this period the pain in the ear and face continued severe, morphia and conium were given internally. Aconitine and belladonna were applied in solution to the external auditory canal, and chloroform to the face with some relief. The day following the division of the nerve the profuse flow of saliva had ceased, and liquid and semi-solid food could be taken freely without causing pain. Considerable portions of the ulcer were subsequently sloughed off by ligature without the slightest pain to the patient; but the swollen and infiltrated condition of the inferior part and base of the tongue, precluded the possibility of enclosing within ligatures the whole of the disease. Exactly one month from the date of the division of the nerve, she began to feel again on the left side, at the tip of the tongue. The flow of saliva, became again increased, and the surface of the deep excavation in the tongue began again to be somewhat painful to the touch. After a consultation with my colleagues, and taking into consideration the depressed condition of the patient's health, the extension of the disease of the tongue beyond the median line, and the increasing size of the cervical gland, it was deemed advisable not to employ any further operative means.

[On the 7th of July, the patient died. Some slight arterial hemorrhage took place from her mouth a few days before her death. No complete post-mortem examination was allowed.]

This operation, of dividing the sensitive nerve of the tongue was adopted for the purposes alluded to in the report of the details of the case.

1st. To destroy the sensibility of the ulcer, so as to enable the patient to take food.

2nd. To reduce the flow of saliva, which was excessively distressing.

3rd. In the hope that the pain she experienced over the distribution of the fifth nerve might be immediately relieved. And

4th. To deprive the operation of sloughing off the diseased portion of the tongue by ligature void of pain.

The first and second of these intentions were accomplished by the operation; the third was only in part accomplished, and, I suspect, from the divided nerve (exposed in the wound made in the operation) being irritated or inflamed by the contact of the saliva, mixed with the secretion from the ulcer. The fourth was also in part accomplished, and, I believe, might have been entirely so, had not the supervention of local inflammation in the adjoining structure retarded the intended operation by ligature, and so allowed the disease to progress to such an extent, as to preclude any attempt at its complete removal.

Experience alone can determine the question as to whether the division or excision of a piece of the sensitive nerve of the tongue may be justifiable in cases of painful ulcers on that organ, when not remediable or relieved by other means; but when a large portion of the tongue is to be removed by ligature, I would earnestly suggest the previous division of the lingual gustatory nerve, so as to deprive that method of removal (by ligature) of its greatest objection, viz., the suffering to the patient, which is necessarily associated with it when the nerve is entire.

The case here detailed is interesting in reference to the short period of time, one month only, required for the reunion of the divided nerve to become so far established, that the patient could feel, indistinctly, mechanical impression made upon the previously insensible left side of the tongue.

By referring to Mr. Rose's report, it will be seen that, on June 21st, two months after quitting Guy's, the patient states, "For the last month, sensation has been gradually returning more and more," so that we may presume the nerve had not become completely united before that time, about two months from the period of its division.—*Guy's Hospital Reports*, Vol. VII., *p.* 254.

74.—*Hare-Lip in France.*—It is, perhaps, not generally known in this country, that French surgeons are in the habit of operating upon children for this deformity a few days after birth. The justly celebrated Paul Dubois is a great advocate for this practice, which, it is contended, is extremely advantageous, as children at that early period, do not struggle; union is obtained very rapidly, and the little creatures cry but little, take the breast well, and do not seem to suffer much. M. Guersant, surgeon to the children's hospital, stated the other day, before the surgical society of Paris, that out of seven children operated upon almost immediately after birth, he failed only once; whilst out of the same number of children one month old, he failed five times. He attributes this disproportion to the fact, that newly-born children can do without the breast during four days; (?) they thereby are not apt to tear open the sutures by efforts at suction.—*Lancet*, *June* 8, 1850, *p.* 694.

75.—*On the Excision of Enlarged Tonsils.* By PROFESSOR SYME, Edinburgh.—[Mr. Syme remarks that childhood is the age most exposed to this morbid condition, though it not unfrequently makes its appearance during adolescence, and is sometimes met with at a considerably more advanced period of life. The only source of speedy and effectual relief is local treatment, for which the means that at different times have been of use are escharotics, ligature, and excision. After making some remarks on the antiquity of each of these, Mr. Syme says,]

In the year 1821, during my residence in the Royal Infirmary of Edinburgh, as house surgeon, one of the female patients suffered from enlargement of a tonsil. After trying sulphate of copper, and similar applications, without success, I attempted the ligature, but without being able to accomplish it, and then as the tumour was of no great size resolved to remove it by excision, which was accordingly done very readily by means of a hook and curved scissors. This, so far as I know, was the first instance of a tonsil being cut out in Edinburgh. Next year I went to Paris, and there found M. Lisfranc in his course of operative surgery on the dead body, treating excision of the tonsil as an established operation. He employed for the purpose a straight narrow blunt-pointed bistoury, of which the blade was sheathed, except for rather more than an inch from the point, and explained that if the tumour was pulled inwards from the side of the fauces or merely held steady in its natural position, the knife might be carried with perfect safety through its base, as the blade must then be parallel with the carotid artery, and at such a distance from it as to render any injury of the vessel quite impossible.

Much impressed with the advantages of the operation as thus performed, I endeavoured, on my return home, to promote its adoption, and having carried with me the forceps of Muzeux, placed them in the hands of Mr. Liston, who also ever afterwards did all in his power, by precept as well as example, to establish the practice. The counties of Perth, Stirling, Peebles, and Roxburgh, are very prolific of enlarged tonsils, and the success attending their removal by excision soon brought patients from all parts of the country, on both sides of the Tweed. Abundant opportunity has consequently been afforded for determining the merits of the operation on the sure ground of extensive and long continued experience.

In no instance has hemorrhage or any other unpleasant accident occurred. The texture concerned possesses so little sensibility that the excision is accomplished with hardly any pain; and children, who are sometimes induced with difficulty to submit, in the first instance, seldom offer any objection to removal of the second tonsil. I never employ a speculum of any sort,—even in the youngest and most obstreperous patients,—the forceps sufficiently keeping down the tongue, and the wide opening of the mouth preparatory to the emission of a cry, affording ample space for the knife to act. In the whole course of my practice I have only on two occasions failed in accomplishing the object in view, through the excessive size of the tongue and contraction of the mouth. The knife should possess a very keen edge, as the glandular substance is sometimes indurated, and a partial division of it would lead to great embarrassment, from the blood trickling into the pharynx, and causing such cough or convulsive efforts as must render a fresh seizure of the tonsil next to impossible. I generally remove the left tumour first, and then the right one, by crossing my hands so as still to retain the knife in the right. The process is instantaneous, and the relief immediate. The bleeding seldom exceeds a teaspoonful or two, and no after treatment is required.—*Monthly Journal of Med. Science, Sept.*, 1850, *p.* 225.

76.—*Instrument for Arresting Epistaxis.* By M. Gariel.—[This is a tube made of caoutchouc, carrying at its extremity a dilatable balloon, which, when introduced into the nostrils in its undistended state, may, by the process of insufflation, be made to assume such dimensions, and exerting such pressure as completely to arrest the hemorrhage.]

A simple method of plugging the posterior nares suggests itself in examining this tube with dilatable extremity. This operation, as at present performed, whether with a special apparatus or with an ordinary catheter, is frequently very troublesome, though simple in appearance. If the tube be introduced from before backwards through the cavities of the nose, until it has quite cleared the posterior nares and arrived in the pharynx, and be then dilated and drawn forwards, we obtain a more complete and manageable plug than that usually made of lint.—*Dub. Quarterly Journal of Med. Science, Aug.*, 1850, *p.* 129.

77.—*Stricture of the Rectum.* Reported by Dr. Charles Kidd.—[This was a case of stricture of the rectum which was divided by Mr. Simon, at St. Thomas's Hospital. The main features of the case were as follows. Eliza L., æt. 30, married, had been under the care of Mr. Simon for disease of the rectum for two years, attended by stricture. Having suffered from syphilis extensively, the conclusion arrived at was that it might have arisen from inflammation of a syphilitic origin and not malignant. After describing the symptoms consequent on the affection, and her appearance as presenting all the marks of great emaciation and suffering, Dr. Kidd proceeds to say:]

" As usual in bad cases, the hemorrhoidal veins have become engorged, and the cellular tissue about the lower end of the gut also extensively diseased; so that, together with piles, some of her recent symptoms would indicate the existence also of fistulæ. On examination before the operation, the stricture was felt very distinctly, impermeable to the end of the little finger, and very high up,—a point of corroborative interest in the diagnosis, *non*-malignant stricture being generally so situated. even as high as the sigmoid flexure, where it joins the rectum; the reverse, perhaps generally obtaining in carcinoma, which is situated among the glands about the anus. The diagnosis, I need scarcely observe, of the nature of the disease in this case, required a great deal of care. Mr. Simon's long acquaintance with its history, however, left no doubt on his mind of its non-malignant character.

All the usual palliative treatment having failed out of hospital, Mr. Simon, on Saturday, advised the operation. The length of time, indeed, the system seems to bear up against this disease. is not the least of its peculiarities. Once it breaks down, it is a *facilis descensus* indeed; the patient gets worn out and emaciated,—purulent matter, of a scalding nature, comes constantly away,—the pulse gets small and intermittent,—the stricture perhaps ulcerates,—and the wretched patient is worn out. To avoid all this, the certain result of not operating, she was induced to make up her mind. Though her face exhibited much nervous anxiety and suffering, her general health had not, perhaps, given way.

Having been placed under the influence of chloroform, she was prepared for the operation. Placed on her face, the stricture was soon reached, the mere point of the finger getting into it, and giving rise to much mucous discharge, evidently high up; a grooved director was passed up to it, together with a bistoury. The director was next let fall out, and the knife carried through all the parts up to the coccyx. The operation is obviously of great extent; the lower part of the rectum being destitute of peritoneum, makes it, perhaps, less serious than it otherwise would be. The intestine above the point of stricture seemed quite free. The rectum was laid open, the muscular tunic, on division, resembling not a little the structure of the œsophagus. Two vessels, possibly branches of the superior hemorrhoidal, spouted out pretty freely, and were tied; the fibres of the upper and lower sphincter were also visible. The thick mucous membrane with the stricture; the various veins and nerves were all divided.

After the hæmorrhage had ceased, a large plug of lint was put into the wound, and secured by a T bandage. The chloroform began to lose its effect, and she was removed to bed.

The operation differs, only perhaps in extent, from that for fistula in ano; it is of interest, as differing from Amussat's operation for stricture of the rectum, which consists in cutting into the cavity of the intestine from above, and making an artificial anus in the groin,—a remedy little to be preferred to the disease. It is of interest, also, as exhibiting the resources of art in a disease not malignant, and when all other means seemed of no avail.

Much care is necessary, of course, in distinguishing stricture of the rectum. An enlarged or displaced uterus, or, in man, an enlarged prostate may give rise to all the symptoms of this disease; sarcomatous tumours, not unfrequent in the rectum, should also be borne in mind; together with the long catalogue of malignant tumours to which this part of the system is specially liable, the latter form by far the largest proportion of diseases of the lower bowel, attended with stricture.

The whole length of the rectum, I may add, may be diseased, or two particular and distant points. We may have mucous and purulent discharges from the suppuration of piles, spasmodic contraction of the sphincter, and strangulation of piles; in all these the mode of operation will be different. Need I say, how many cases are aggravated by neglect; the mucous, cellular, and muscular coats all extensively diseased, the disease creeping steadily on, some other part of the system apparently engaged; many people dying of different intestinal affections, as they think, the disease, after death, found in the rectum.

As a case essentially of a practical character, indeed, I think the present one is worthy of our attention. After the operation, Mr. Simon ordered a large dose of opium, to allow the parts to rest. Since then the woman has been doing well, and no abdominal pain whatever.

The following day the plug was taken out—there appeared to have been no hemorrhage.

26th.—Going on well, the bowels acting comfortably.—*Med. Times, July* 27, 1850, *p.* 89.

78.—*On Fungous Tumour of the Rectum in Children, attended with Bloody Discharges.* By M. LECLAYSE.—M. Martin has already directed attention to the affection as it occurs in the adult, producing discharges which are mistaken for those from hemorrhoids. The first case occurred in a child, æt. 5, about whom the author was consulted in consequence of hemorrhages which occurred during a prolapsus ani, and which arose from an excrescence that he at first mistook for hemorrhoids. Examining it more closely, he found it was a spongy vegetation, not unlike a portion of the placenta, which protruded from beyond the sphincter when the child went to stool, and was quite insensible to the touch. As the hemorrhage had been considerable, the fungus was touched with the nitrate of silver, whenever it protruded; and, owing to its softness, four or five applications, at intervals of several hours, sufficed for its destruction. In a second case, a girl, æt. 8, had become much reduced by the quantity of blood she had lost during several weeks; and a fungous tumour, about the size of an almond, was easily removed in the same way. A third case occurred in an infant six months old, in whom efforts at stool protruded a tumour the size of a pea, which bled. The author, believing it to be the germ of the fungous tumour, also treated it with caustic.

M. Leclayse believes that this affection is often mistaken for hemorrhoids; and especially when the bleedings are said to be due to internal piles. The caustic could not be applied very high up, but as the bleeding has only occurred on the protrusion of the tumour, this has been easily reached, the application being successful even when the base of the tumour could not be attained.—*Rev. Méd.-Chirurg.—Brit. and For. Med.-Chirurg. Review, October,* 1850, *p.* 552.

79.—OPERATION FOR PROLAPSUS ANI.

By Professor J. F. DIEFFENBACH.

[M. Dieffenbach followed four distinct methods according to the nature of the case. The preliminary treatment in all consisting in a dose of castor oil the day before, and an enema of warm water immediately preceding the operation. The patient is bent forward over a table, the nates being separated by an assistant.]

1. *Diminution of the anal opening by excision of folds around it.*—After returning the rectum, one of the cutaneous folds which surround

the anus like radii is seized by a pair of sharp-hooked forceps half an inch from the opening; and the fold is then cut away close within the opening by a pair of straight sharp-pointed scissors. Four, five, or even more strips are successively removed in the same manner, so that the anus is surrounded by a wreath of bleeding rays. The more lax the skin, the broader must be the strips removed. The wounds must be at equal distances from each other, for when two are close together, the small intervening portion of skin is easily destroyed by suppuration, and one wound results, which is so large, that the subsequent cicatrix and stretching of the skin are injurious. Cold water dressing is then employed. In slight cases where the prolapsus only consists of one or two folds of mucous membrane which can easily be replaced, this plan is to be adopted, and it is generally successful. In more severe cases it is either useless, or but of temporary utility.

2. *Excision of wedge-shaped pieces from the anus* is recommended when this part is greatly weakened, but not entirely paralyzed, and when the prolapsus consists of large, fresh, unaltered, folds of mucous membrane, which only project during the act of defecation. When the projection is only on one side, but one wedge is excised; when it is on both sides, or when it forms a sort of ring, a wedge must be removed from each side. The prolapsus is returned; a sharp hook is passed within the anus as far as the edge of the part returned, and carried through the membrane and first fibres of the internal sphincter ani; the hook is drawn a little outwards. and the wedge cut away with a small scalpel. When the prolapsus is not great, the wedge may be small; but in more severe cases, Dieffenbach has often excised two wedges, an inch wide on each side. The edges of the wounds are immediately brought together by strong sutures, and cold-water dressing used. The result is most satisfactory, the cure complete, and relapse never occurred in Dieffenbach's practice.

3. *Excision of parts of the anal ring and of the callous prolapsus* is indicated when the opening is very much widened. the prolapsus old and large, the folds of mucous membrane dense, firm, and hard, or loose and spongy, with large permeating veins, the patient having suffered from continual hemorrhage. The operations upon old and hard, or soft and spongy prolapsus differ, but in both the part must be protruded at the time of operating. In the first class of cases, the projecting fold is seized by a pair of forceps, and a strong suture passed through its base; the ends are kept on opposite sides, while the fold is removed by a pair of scissors, in a semicircular direction around the anus; the ends are then brought together, and serve the purpose of fixing and drawing forwards the edge of the wound, while fine sutures are passed to unite them. This is all that is necessary in cases where the anal opening is not very large. When it is so, a wedge must be excised from it, in the manner described in the preceding paragraph, the two wounds forming together a T. In very severe cases, it is dangerous to do too much at once, and it is far better to complete the operation after two or three intervals. The convolutions having been removed, and the wound healed, after a month or so, when the parient is quite well, the wedge may be excised.

4. *Extirpation of the spongy prolapsus,* which consists of the convolutions of the loose sanguineous mucous membrane of the rectum, is performed in the manner just described, when the prolapsus is partial, and the anal opening narrow. When it is total, and large, the operation is first performed on one side—that of the greatest protrusion—in a different manner. The part is seized by a pair of curved forceps, resembling those called entropium forceps, and the convolution is then cut away by scissors, close to the edge of the forceps. The edges of the wound are immediately closed by fine sutures, as severe hemorrhage often comes on. The part removed includes the centre of the prolapsus, and extends at one end to the skin surrounding the anus, and at the other to the mucous membrane of the rectum. The rectum is cleaned by injections of cold water, the patient kept in bed, and cold applications used. An opiate is given to prevent defecation during the first day. Sometimes this alone effects a cure; in other cases it is necessary to repeat the operation on the opposite side, after the patient has quite recovered from the effects of the first. Dieffenbach relates some remarkable cases, in which patients who had been extremely reduced by continual bleeding and suffering, were thus restored to perfect health.

5. *Cauterization.*—When the external and internal sphincters of the anus are paralysed, the anal opening is often so large, that a doubled fist can easily be passed through it, and the intestine can only be kept within it by some mechanical contrivance. Removal of parts of the prolapsed intestine would be useless in such cases, as fresh protrusion would continually take place. Excision of parts of the borders of the opening also proves useless, as the cicatrices soon become soft and yielding. The actual cautery, however, suffices to effect a cure, acting partly upon the nerves, and partly by the formation of contracting cicatrices. The anus being protected by the introduction of a ball of charpie, a cauterizing iron, of the size of a large walnut, is applied upon the skin surrounding the anus, and repeatedly carried slowly around the opening, so that an eschar is formed, an inch or an inch and a half in width, and the skin appears as if burnt to leather. The iron must be carried close to the line of connexion between the skin and mucous membrane. The charpie is removed from the anus, a smaller quantity introduced, and a wad of cotton-wool laid upon the eschar. The patient is placed in bed, and an opiate given. Cotton wool is applied until separation of the eschar at the edges commences. Stimulating poultices are then used. After complete separation, the suppurating surface is dressed simply, and castor-oil given. When cicatrization is complete, the patient is perfectly cured. Dieffenbach has found the opening so much narrowed, that bougies had to be used for some weeks; but this is rare, and the general result is most gratifying, both to surgeon and patient.—*Brit. and For. Med.-Chirurg. Review, October,*1850, *p* 295.

80.—*On Linseed Oil in Hemorrhoids.* By M. VAN RYN.—M. Van Ryn believes, that, in general, surgical treatment is too hastily resorted to in this affection, and he wishes to bring under the notice of the profession a remedy he has found of great efficacy during twenty-five years.

It consists in the administration of two ounces of fresh linseed oil morning and evening; and so rapid is the amendment generally, that the remedy is seldom continued longer than a week. Sometimes the stools are somewhat increased in quantity, but neither vomiting nor any other ill effect is produced. The only precaution the while, is the abstinence from alcoholic drinks and too stimulating a diet.—*L'Union Médicale.*—*Brit. and For. Med.-Chirurg. Review, October*, 1850, p. 553.

URINARY ORGANS.

81.—THREE CASES OF PERINEAL SECTION.

Under the care of Mr. Cock, at Guy's Hospital.

[Mr. Syme's method of perineal section for the cure of stricture being here put into practice, it is hoped these cases will be of great interest and value. The first is the case of a waterman, aged forty years old, who, at the age of fifteen, contracted gonorrhœa, and who, for the whole of the interval, from this period to that of his admission, was the victim of catheterism, either by regular or irregular practitioners; so that, at the time of admission, the urethra could only be kept permeable by a frequent introduction of the bougie, accompanied by much pain and suffering, occlusion of the canal rapidly taking place if it was abandoned for a short time.]

Under these circumstances of constant misery, the poor man expressed himself desirous of submitting to any operation which would afford him the chance of permanent relief; and Mr. Cock considered that the only means available in his peculiar case, consisted in dividing the diseased and contracted portion of the canal, thereby allowing the urine to pass for a time through the opening in the perineum, and giving an opportunity for the urethra to regain a healthy state. If the latter condition had not been accomplished, it was Mr. Cock's intention to maintain a permanent opening in the perineum, below the seat of mischief, the moderate inconvenience of which would have been a preferable alternative to the distress which the patient constantly endured. The latter, however, was a man of most irritable and nervous temperament, and although the operation was undertaken at his own request, he looked forward to it with the greatest anxiety and apprehension.

On March 20th, after the patient had been placed under the influence of chloroform, Mr. Cock introduced a small grooved staff into his bladder, and divided about two inches of the urethra, including the bulbous, and a part of the membranous portions of that canal. An elastic catheter was introduced, and the patient was taken to bed in a singularly depressed condition.

The loss of several ounces of blood a few hours after the operation increased the prostration, from which he never rallied. The next day his irritability became extreme, and he could not bear the presence of the catheter. He complained of general pain of his body and extremities, difficulty of breathing, rigors, and intense restlessness. The various symptoms which characterise phlebitis soon came on, and although all available means were adopted to combat the disease, he continued to get worse. On the afternoon of the 25th his arms suddenly became swollen and tense, apparently from general interstitial effusion, accompanied by severe pain. He now sank rapidly, and died in the night, five days after the performance of the operation.

The *post-mortem examination* was conducted by Mr. Birkett, who furnished the following account:—The cuticle was separated from the cutis along the fore-arms, and underneath was serum; these parts were much discoloured, but the left arm was most affected. Both lungs were infiltrated with serum, but no acute inflammation existed. The heart was normal, as also the alimentary canal; the liver congested, soft, and mottled. Both kidneys were congested and coarse, the two structures very indistinctly marked, and the tunics adherent. The pelvis of the left kidney was very much injected, purulent fluid was expressible, and in the left organ some parts were rather soft. The bladder was not very large, nor were its walls thick; the mucous membrane was, however, much ecchymosed in isolated patches. The edges of the incision through the perineum were sloughy, and all the veins forming the left prostatic plexus, were more or less filled with coagula, in some parts adherent to the lining membrane of the vessels, but no pus was detected. Some of the veins constituting the right plexus were likewise inflamed; no traces of peritonitis were discovered. Though this patient was very irritable and nervous, it should be borne in mind, that at the time of his death phlebitis reigned with great violence in the hospital.

The second case runs as follows:—William G——, an excavator, aged thirty seven, was admitted into Stephen ward, April 3rd, 1850, under the care of Mr. Cock. Has had stricture twelve years, which he attributes to previous attacks of gonorrhœa. The complaint appears to have been much aggravated by several severe courses of catheterism, each of which left him worse than he was before. Three years ago, he was under M. Ricord, at Paris. His urinary difficulties have continued to increase up to the present time, and he is now suffering from all the symptoms of confirmed stricture. On examination, the obstruction was found to be situated near the root of the penis, anterior to the scrotum; the callosity surrounding the stricture could be distinctly felt from the exterior, and the canal just allowed the passage of a probe.

Mr. Cock, on the 10th of April, divided the stricture, including about an inch of the urethra, on a grooved probe; No. 6, elastic catheter was then passed into the bladder, and retained two days, when it produced prostatic irritation, which was subsequently followed by an attack of testitis. Nos. 7 and 8 catheter were afterwards occasionally passed, and the urine continued to ooze from the wound during micturition for nearly three weeks, when the aperture finally closed. The patient left the hospital May 18th, forty-five days after admission, quite well, and able to pass his water in a good stream, which he had not done for many years.

We now proceed to give the third case. Thomas T——, a labourer, forty-four years of age, was admitted into Philip ward, April 9th, 1850, under the care of Mr. Cock. The patient has been married twenty-four years, has suffered eighteen from stricture, and ascribes his affliction to a previous gonorrhœa. He has had retention of urine many times, and has undergone various severe courses of catheterism. During the last four years, the patient has come occasionally under Mr. Cock's observation. A considerable portion of the urethra appeared to have been preternaturally contracted, but the principal obstruction was a few inches down that canal. On several occasions, a certain amount of dilatation has been effected with perfect, but temporary relief to his symptoms, a relapse having always followed the cessation of the means employed. Much injury has frequently been done by the injudicious use of instruments.

On examination after his admission, an almost total occlusion of the canal presented itself, about four inches down, just at the junction of the penis with the scrotum; and the induration surrounding the stricture was readily felt from the exterior. The obstruction was only permeable to a fine probe, after which, the smallest-sized bougie followed, and readily found its way into the bladder.

On April 17th, Mr. Cock divided the stricture, including about an inch and a half of the urethra, on a grooved probe, after which, a small-sized elastic catheter was guided through the more deeply-seated obstructions into the bladder, and retained three days. Larger catheters were successively passed at intervals until the canal had become sufficiently dilated. The wound suppurated freely, and remained patulous and gaping, so as to afford a free view of an instrument as it lay in the passage. Thirty-five days after the operation the incision was all but healed, merely allowing the escape of a drop or two of urine during micturition. The patient passed his water in a perfectly good stream, and a No. 7 catheter could be readily introduced into the bladder. All his former symptoms of distress, intolerance, &c., gradually disappeared, and he was discharged May 28th, 1850, with complete closure of the wound, in a very favourable condition of health, and passing his urine without any difficulty."—*Lancet, June* 29, 1850, *p.* 775.

82.—*Case of Stricture of the Urethra, involving the greater portion of the Canal—Complicated with Stone—Perineal Section—Subsequent Treatment of Stricture by Internal Incision.* Under the care of W. Fergusson, Esq., reported by H. Smith, Esq.—[As there has been lately so much discussion as to the merits of certain measures adopted for the cure of stricture, the following complicated and difficult case cannot be read without interest.]

As far back as the beginning of April, the patient, who is a young, sickly-looking man, about twenty-five years of age, was admitted into the hospital under the care of Mr. Fergusson. He was in a condition which was much to be pitied. His urine was coming from his bladder in three different directions; rather, it was passed by three separate outlets. Only a few drops came away by the urethra whenever he attempted to urinate. The greater portion was evacuated through two artificial openings, one in the perineum, the other in the abdominal wall, on the left side above the ilium. And from the latter it was emitted on voluntary exertion, in a complete jet, as from a fountain. A stricture commenced between two and three inches from the meatus, and apparently, from the thickening of the parts, extending all along the spongy urethra to the bulb. The patient was miserably weak and hectic, and in constant suffering from the irritability of his bladder, which caused him to rise every hour or two in the night. Besides the opening in the abdominal wall before mentioned, a large scar was observable just above the pubes. The explanation of all this will be found in the history of this case, which is as follows:—Two years since he fell from the rigging of a vessel, and came across a spar upon his perineum. Great pain was caused immediately after the accident, but in a few hours it ceased; some slight swelling, however, soon came on. Nevertheless he continued at his work for a month afterwards, at the end of which period an attack of retention of urine took place, and continued for ninety-eight hours, when a surgeon relieved him by *puncturing the bladder above the pubes.* A canula was kept in the wound for six months after this, and during the whole time the patient only passed a few drops of urine by his urethra. Soon after the operation some leeches were applied to the belly, and two of the bites, one on each side, degenerated into fistulous openings, which allowed urine to pass through; that on the right side eventually closed up, but the one on the left has continued open ever since. At the time the bladder was punctured, *an incision was made through the perineum* (rather into the perineum), but nothing but pus was let out. Six months after, the water came away by this opening as well, and has been passing ever since. In such a state, then, as has been described, he was sent to Mr. Fergusson. This gentleman very carefully examined the urethra, and found that the smallest instrument was stopped about two and a half inches from the meatus, and that with great care it was impossible to get into the bladder through the natural passage without using force. It was very evident that the first thing to be done was, to get a free passage for the urine, the retention of which, and the constant irritation caused by its trickling through the three outlets, were telling so much upon the man's health. Mr. Fergusson therefore determined, after allowing some interval for the patient to recruit his health, to open up the perineum freely, and pass an instrument into the bladder. Consequently, on Saturday, April 20, the man was brought into the theatre, placed in the position for lithotomy, and a free incision was made through the perineum down upon the urethra. A short gum catheter was then with ease introduced by this wound into the urethra, and from thence to the bladder. Whilst Mr. Fergusson was doing this, he found out a new and unexpected complication; he felt the instrument strike against a hard substance; suitable instruments were obtained, and the foreign body was removed: it was found to be a calculus about the size of a nut. It appeared to have been formed in a sort of pouch, not in the bladder, but in front of its neck. A free opening thus having been made for the urine to escape, the patient was sent up to his ward.

For some days after this proceeding, the patient went on very well.

The urine came freely away through the wound in the perineum. No instrument, however, could be passed through the urethra. At the end of a week, he was somewhat suddenly attacked with unfavourable symptoms; he vomited; his skin became jaundiced, and he complained of great pain in the abdomen, more particularly over the seat of the liver; and on examination, this organ was found to extend beyond its normal limits, and it was evident that the cause of his symptoms existed in an affection of this viscus. Some anxiety was entertained respecting the result of this attack, for upwards of a fortnight, as the patient became excessively depressed, and was unable to take much nourishment. By great care, however, the symptoms were removed, the inflammatory attack subsided, and there was so much improvement, that, on May 10th, Mr. Fergusson thought fit to begin to treat the stricture again. He endeavoured very patiently to introduce a small catheter, but he could make no impression, and, as there was considerable induration along the urethra outside, he applied the potassa fusa, at the point of a wax bougie, to the face of the stricture. The patient was enabled to take his nourishment well, which is chiefly fluid, beef-tea, wine, &c.

May 25th.—This man has improved to such a degree that he is enabled to sit up; and, although his general health is still weak, he is much better than he was. The benefit which accrued to him from the operation of opening the perineum is evidenced by the state of his bladder; there is much less irritability now that there is a free discharge for the urine. This opening has been kept patent by the introduction, from time to time, of an elastic catheter. Mr. Fergusson has not yet been able to get any instrument through the urethra; although he has repeated the potassa fusa two or three times, the only benefit which appears to have arisen from the caustic, is a partial removal of the induration outside the urethra.

May 29.—As there appeared no possibility of getting an instrument through the strictured portion of the urethra by ordinary means, Mr. Fergusson passed an instrument down to the stricture, and divided it pretty freely from within. This instrument is Mr. Fergusson's own invention; it consists of a grooved director, with a handle to it, and a long narrow knife with the cutting edge at its very extremity. The blade is so narrow, and the groove in the director is so deep, that the cutting edge of the knife does not come into contact with the healthy part of the canal, but can be made to freely incise the strictured portion. Two days after this operation, a No. 2 catheter was passed right into the bladder with comparative ease.

June 6th.—Matters are going on satisfactorily. The opening in the abdominal wall has closed up; and, although he cannot pass his water by the urethra in a stream, it dribbles away quickly. Mr. Fergusson was enabled yesterday to pass No. 6 catheter. His health is still unsatisfactory; his appetite remaining bad.

13th.—There has been so much improvement during the last week, that this young man has been able to go out and take a walk in the yard of the hospital. The opening in the perineum is not closed, and it allows the urine to come away through it pretty freely yet. Mr. Fergusson passed to-day a gum catheter through this into the bladder, and a No. 4 silver instrument through the urethra.

27th.—Mr. Fergusson has found some difficulty, during the last few days, in introducing a No. 5 catheter through the stricture; it appears to have contracted somewhat again. He has passed both No. 4 and No. 5 to-day; but it is evident that recontraction of the divided stricture is taking place already; and it is Mr. Fergusson's intention to have the patient placed under chloroform, and then to divide the strictured portion much more freely from within than he had done before; and subsequently to introduce a gum catheter, and retain it for some time. The patient has been enabled to go out and take a trip down the river in a steam-boat.

July 6th.—Mr. Fergusson freely divided the stricture with the urethrotome, and passed No. 8 catheter, and allowed it to remain in the bladder.

9th.—No. 10 elastic catheter was passed, and ordered to be kept in. The patient suffers very little from the presence of the instrument.

16th.—The catheter has been retained with the production of scarcely any irritation; and, when Mr. Fergusson saw the patient to-day, it was quite loose in the urethra. This therefore was taken out, and as large a one as the urethra could with facility admit was introduced, and is to be kept in. The opening in the perineum is not entirely closed up. The general health of the patient is still weakly. He feels that the necessary confinement to his bed is a drawback to him.

22nd.—As the patient expressed a desire to return to his home, and as the urethra was now fully dilated, he was dismissed to-day. - *Med. Times, July* 27, 1850, *p.* 90.

83.—STRICTURE IMPASSABLE TO INSTRUMENTS—COMPLICATED WITH PERINEAL FISTULA -PERINEAL SECTION.

By W. Fergusson, Esq., King's College, London.

[The patient in this case when a young man had gonorrhœa, and about seven years ago he first began to feel the symptoms of stricture. These increased, when about fifteen months ago, effusion of urine took place suddenly, and he was taken to one of the London hospitals, where incisions were made into the perineum. A fistula remained, however, from these openings, and at the end of nine months he was again admitted, as the symptoms of stricture continued to be aggravated. A decided attempt was now made to cure the stricture by dilatation, by the introduction and retention of a large-sized instrument, but it failed, and the patient some time after sought admission into King's College Hospital. The scrotum was then found immensely swollen, with some fluid. The catheter could not be passed, and there was a fistulous opening in the perineum. Incisions were made into the swelling, and a large quantity of fetid urine escaped. As no catheter could be passed, Mr. Fergusson decided upon laying open the urethra by external incision.]

On Saturday, July 6th, Mr. Fergusson performed the operation in the following manner:—The patient being put under the influence of chloroform, a catheter was passed down to the seat of stricture; a free incision was then made through the indurated tissues in the centre of the perineum down upon the urethra, and the point of the catheter was reached. The knife was now carried further along, and the whole extent of the strictured part was divided, until the surgeon was enabled to pass an instrument fairly into the bladder. There was no particular amount of bleeding, and no more difficulty in the operation than usual. The patient was placed in bed, and a gum-elastic catheter was retained in his bladder.

8th.—This patient has not had a bad symptom since the operation, and has suffered only the ordinary amount of sympathetic fever: pulse 96, tongue clean; the catheter is producing no irritation, and the water passes freely away through it.

13th.—The catheter has been producing some amount of irritation: it was, in consequence, taken out of the bladder; but there was some little difficulty in withdrawing it, as its extremity was encrusted with phosphatic deposits. Mr. Fergusson introduced another instrument, and ordered it be retained.

16th.—Mr. Fergusson to day introduced a No. 12 catheter into the bladder with ease. The wound is looking well, and is granulating in a healthy manner: the patient is improving rapidly in his health; appetite is good, tongue clean, pulse good, and he is recovering fast. The catheter which was last passed could only be retained two or three days, as it produced considerable œdema of the prepuce; it was accordingly extracted, and the parts were allowed to remain perfectly quiet, and no other instrument was introduced. After this the wound rapidly healed. On the 22nd no water at all was coming through the perineal opening, and he could pass it along the urethra in a full stream. Everything continued going on well with this patient: an instrument of full size was introduced along the urethra once or twice more, and he was dismissed well in the middle of August.

This case presents, in many respects, features of interest, and especially as lately there has been considerable discussion as to the line of treatment which should be adopted in certain cases of inveterate stricture which will not yield to ordinary measures. Surgeons of all ages, and of every school in this country, have recommended that those cases which will allow of their removal by dilatation alone should be treated by the catheter; and the majority of cases which we have to contend with, either in hospital or private practice, are of this nature,—that is to say, in by far the greater number of cases of stricture of the urethra, a temporary cure, at least, can be brought about by the cautious employment of dilating force. But hitherto the most experienced and careful surgeons have now and then met with such obstinate cases of stricture—cases not only obstinate, but complicated to such a degree, either in consequence of their situation or extent, that they have found it absolutely necessary to resort to stronger measures than those afforded by the use of the catheter or bougie. Thus every now and then the surgeon meets with a stricture involving the orifice of the urethra or extending some way within it. In such an instance it is almost useless to attempt a cure by dilatation alone: the readiest way is to divide the stricture by a cutting instrument, and then to apply a dilating power subsequently: by this means a cure will be rapidly brought about by an exceedingly simple operation.

Then, again, there are a class of cases which are most troublesome to treat, and which are likely to lead to the most disastrous results to the patient, if some permanent relief is not given; and they generally, if not attended to, go on to such an extent, that it is found needful to lay aside the bougie, and to employ the knife. The early history of the present patient is an instance of this. A stricture is met with, and forms about the bend of the urethra. No care is taken about it; the person indulges in his usual habits; year to year is passed in this manner; the obstruction increases in severity and extent; and, if application is made to a surgeon, in all probability no instrument can be passed through the stricture, either in consequence of its extent and hardness, or from the circumstance of the urethra being in a most irritable condition. Under such circumstances there is constant danger of the urethra giving way behind the strictured part, in consequence of the straining efforts the patient feels compelled to make. This does happen suddenly, and he is either carried off, or, by timely interference, he rallies; but a fistulous opening remains in his perineum, and, as an outlet is thus produced for the urine, some relief is obtained; the patient again goes about his occupation without attending to the disease. The continual trickling of the urine through the fistulous opening produces a chronic inflammation and thickening of the tissues of the perineum; the urethral canal becomes more and more obstructed, and at last the condition of the patient becomes so bad that he is obliged to seek relief.

In some of these cases, if there be no particular urgency of symptoms, and if the obstructed portion of the urethra is not very extensive, and there be not very great induration of the tissues of the perineum, an entrance may be obtained into the bladder by the catheter, if both surgeon and patient persevere for a sufficient period. And when this is effected, and the urethra has become fully dilated, the fistulous sinuses will close up, and all the bad symptoms resulting from the obstruction of the canal, and from the unnatural course which the urine had been taking, will disappear.

But, in some of these cases, matters have become so bad, that recourse must be had to some more powerful agent than the catheter. In consequence of ulceration having taken place behind the stricture, extravasation of urine occurs, and places the patient's life in jeopardy: if he recover from the first attack, and yet no instrument has been got into the bladder, or the case has been neglected, he is liable to its recurrence, as in the case of the patient just narrated. On two occasions this serious accident took place; and, on the last, an enormous quantity of pus and urine was evacuated by the surgeon. As the patient, therefore, had already suffered so much, and as all the evil consequences of neglected stricture had developed themselves, and, after careful attempts, it was found impossible to get an instrument into the bladder by fair and gentle catheterism, Mr. Fergusson considered it a proper case in which to perform the operation of external incision. He had had several instances in the hospital of a somewhat similar nature, when the operation had been followed by the most perfect success. The same good result fortunately took place in this instance. In a short time the urethra had become fully dilated, and the patient left the hospital entirely free from every vestige of the troublesome malady which had annoyed him for so long a time.

This case, together with one reported some time since, will probably excite some interest at this time, as there has of late been a free, and somewhat angry discussion amongst certain parties respecting the propriety of cutting strictures at all. Some eminent authorities, and one in particular (Mr. Syme of Edinburgh), have distinctly affirmed that the division of a stricture at the point of the catheter, in such a case as here narrated, is never necessary and, in fact, is an unwarrantable proceeding on the part of the surgeon. These gentlemen state that no stricture is truly impermeable, and that, with sufficient patience, a surgeon may always get a passage into the bladder. We are by no means disposed to deny this assertion; we believe that a metallic instrument may, by a sufficient amount of force, be carried through into the bladder, however firm the structure may be. There are others, again, who advise somewhat freely the external division of strictures; and it must be confessed that surgeons of the greatest eminence, from Le Dran down to the distinguished Liston, have carried this doctrine into practice in such cases as were deemed proper for such operation.

We have now had an opportunity which perhaps has not fallen to the lot of very many, of observing with accuracy and care the effects of the treatment of stricture by perineal section; and we are now fully impressed with the conviction that neither the one doctrine, that strictures ought never to be cut, nor the other, that perineal section is frequently necessary, and should be unsparingly carried into practice,—is either correct or safe to act upon. We have now seen several cases in which the patient was brought into such a state of misery and danger, and the difficulties in the ordinary treatment were such, that it was found absolutely necessary to resort to the perineal section; and the operation has been followed, as we have before stated, by such excellent results, that any surgeon who would call such practice unwarrantable would either show great prejudice or want of experience. On the other hand, again, we have seen death resulting from the division of the urethra, even in cases where there has been but little difficulty in the operation; and this has happened in instances where it was by no means absolutely necessary to perform perineal section. In one case death resulted ten days after the operation which had been performed, when even a moderate-sized catheter could be passed into the bladder—an unconquerable argument against the performance of perineal incision in cases where a catheter can be previously passed into the bladder, as recommended by Mr. Syme.

As, then, it is found that death does occur after division of a stricture by external section, the surgeon is in duty bound to weigh all the circumstances of any particular case well before he puts this proceeding in force; and we are of opinion that in no instance whatever should the operation of perineal section be performed unless the most careful attempts have been previously made to introduce the catheter, or unless the patient be either in danger, or is particularly anxious to undergo it.

In such a case as we have narrated above, the surgeon is perfectly justified in attempting to make a complete cure of the disease when he finds other means fail; and it will be, for the most part, found that the greatest success after operation ensues in these very cases which present the greatest difficulty, and where the disease has been of long standing, and great disorganisation of the tissues of the perineum and scrotum has occurred; for in such cases the system has, as it were, become accustomed to the irritation which has been constantly kept up, and it will be the better able to bear an operation. But in instances where there there has been disease of a milder character, or of shorter duration, the system has not yet accustomed itself to constitutional irritation; and if a serious operation, as cutting through the perineum into the urethra, be performed, it will readily take alarm, and such excitement will be produced as will be sufficient to carry the patient off. Such, at least, has been the result of our own observation, after watching nearly twenty cases in which perineal section has been performed.

We believe, then, that the safest and most humane part to take in this much discussed matter is to steer a middle course, and to act according to particular circumstances—to operate only in the worst cases, which will yield to no ordinary measures, and by no means to fly hastily to the knife when there is no actual necessity for it.

As for the proposal of Mr. Syme, to cut patients for stricture when an instrument can be already passed into the bladder, we confess it must be looked upon as a proceeding by no means justifiable, except on the urgent demand of the patient himself. We know of instances where fatal results have occurred after this proceeding, and others have been related where it was not only not attended with any good result, but where patients have been rendered worse, in consequence of the non-closure of the opening made during operation, and, of necessity, the existence of a perineal fistula. It appears that even in Edinburgh this method of treating permeable strictures has been denounced, and by a no less eminent surgeon than Prof. Lizars, whose great experience and practical knowledge give great weight to any opinion he may express on so important a matter as the one under notice.—*Med. Gazette, Nov.* 8, 1850, *p.* 816.

84.—CASES OF STRICTURE OF THE URETHRA, TREATED BY EXTERNAL INCISION.

By Dr. JAMES DUNSMURE, Surgeon to the Royal Infirmary, Edinburgh.

[These cases may be of some interest on account of the recent discussions on Mr. Syme's mode of treatment.]

The first case in which I had recourse to it was in the person of an Italian, Joseph Anthony, æt. 48, who was admitted into Ward No. 4, on the 23rd January, 1850. Upon seeing him the day subsequent to his admission,—he having come to the hospital after the hour of visit,—I found him suffering from infiltration of urine into the perineum and scrotum, both of which were much swollen, hard, and inflamed. There were several fistulous openings in the perineum, from which urine, escaped in small quantity when he attempted to micturate. Incisions were made into the scrotum, and one of some extent and depth into the perineum, with the view of opening the dilated part of the urethra behind the stricture. Pus and a considerable quantity of urine escaped at the time. The urgent symptoms produced by the infiltration, and which are so well known that I need not describe them here, gradually subsided, and the parts regained what the patient considered their normal state, —such as they had been in for many years, viz., a thickened and hardened condition of the perineum with various fistulous openings between the verge of the anus and the root of the scrotum. As the inflammatory swelling fell, I found that the hardness extended along the urethra for fully an inch anterior to the scrotum. The incision I had made upon his admission, and which was a little to the left side of the raphé, and in the direction of that for lithotomy, had greatly contracted, and assumed very much the appearance of the other fistulous apertures.

The man was well known to me, and to all those who had been in the habit of attending the hospital, as he had been a patient under Mr. Liston, and also I believe under almost every surgeon who had done duty in the Infirmary since that period. He was addicted to drinking, and had been so for many years. As already mentioned, the more urgent symptoms gradually subsided; and, after various attempts, I succeeded with some difficulty in passing No. 2 catheter through the stricture, which extended from an inch anterior to the scrotum to the middle of the membranous portion of the urethra. Dilatation was continued until No. 7 catheter was introduced. After consultation with my colleagues, and thinking that if the original stricture was divided,—and which I hoped might be anterior to the bulb,—that the remaining hardness of the urethra would subside, I determined upon the external division of that part of the urethra anterior to the bulb, and accordingly, after having administered chloroform, introduced a staff with the groove on the back, and about the same size as No. 7. An incision was then made into the perineum so as to divide the urethra upon the staff from behind forwards, anterior to the bulb, and to the extent of about an inch and a quarter. No. 7 catheter was then passed and retained in the bladder by means of tapes. This was not withdrawn for two days. It was then removed, and No. 9 passed, which was kept in for a short period. In two days more No. 10 was introduced; and, after a similar period, No 12. The thickening and hardness anterior to, and in the middle of the scrotum became considerably less; but there was not much difference to be observed in the perineum, nearly as much urine flowing through the opening made at the time of the operation as that passed by the urethra. No. 12 was introduced every fifth day. About six weeks after the division of the urethra, the patient insisted on being dismissed, which was accordingly done, although the perineal wound, which was not yet healed, still allowed urine to pass.

The difficulty experienced in this case was from the cartilaginous state of the perineum and urethra, and so hard did the textures feel, that it was not easy to distinguish the staff from the surrounding tissues. There was considerable bleeding, but well applied pressure by means of a T bandage was sufficient to arrest it. It did not come from any one vessel, but seemed to be a general oozing from the most superficial to the deepest part of the wound. I have no doubt there was somewhat of a hemorrhagic diathesis, as Dr. Fyfe, my house surgeon, was obliged, from the continuance of the bleeding, to plug the wounds made in the scrotum on the day after the patient's admission.

In such a case as the one now narrated, it was hardly to be expected that Mr. Syme's method would succeed, and although, from the reason mentioned, a partial trial was given to it in the manner above described, yet to have performed the operation that Mr. Syme *recommends*, it would have been necessary to have divided the urethra for nearly the whole of of its extent, which, of course, was out of the question. If the operation did no good, I consider it did no harm, for, although the perineal fistulæ still existed as on admission, yet, had the patient remained under treatment in the hospital, as he was urged to do, I have no doubt they would have contracted and healed under the continued use of the bougie. We can, therefore, in my opinion, draw no conclusion from this case, either for or against the operation by external incision.

The next case was that of William Tucker, æt. 44, who was admitted on the 12th March. He stated that he had laboured under stricture for seven years; that he had applied for relief to various surgeons, and had been a patient for this disease in the Manchester Infirmary, and in two of the London hospitals;—that he was dismissed about nine months ago from one of the latter institutions after having been an inmate of it for nearly three months;—that the largest sized catheter that had been introduced was No. 7;—and that, since leaving the hospital, his symptoms had gradually become worse, and so distressing as to make him apply once more for relief. Upon examining the perineum, a fistulous opening was discovered, which had existed previous to the time he entered the London hospital. Thin purulent matter flowed from this, unmixed with urine, no urinous odour being perceptible. Upon making water the stream was exceedingly small, almost, in fact, in drops. After one or two attempts I succeeded in passing No. 2 catheter. As this was evidently a resilient stricture, and accordingly a proper case for external division, I determined to have recourse to it. No. 2 was then passed a second time, and kept in the bladder for twenty-four hours. It was then taken out, and on the following day a small staff with the groove on the back, and about the same size as No. 2, was passed into the bladder, the patient having previously been put under the influence of chloroform. An incision was then made into the perineum, and I had no difficulty in feeling the thickened part of the urethra, which seemed to be about half an inch in extent, and at the commencement of the membranous portion. Having felt for the healthy urethra behind the stricture, a small bistoury was pushed into it, and carried forwards, until the stricture was entirely cut through, which was known by the grating sensation being no longer felt as the parts were divided. The staff having been withdrawn, No. 8 catheter was passed with ease, and secured in the bladder. The patient was then sent to bed. No bleeding occurred. The catheter was allowed to remain in the bladder for three days (there being no irritation), when it was taken away, and Nos. 10 and 12 introduced; the latter was left in the urethra for about an hour, and then withdrawn. The following day urine was passed by the wound; but by the third this had ceased, and from that time all the water flowed by the natural passage. In ten days from the date of the operation the wound in the perineum had healed. No. 12 was passed for the first fortnight every five days. This instrument seemed to be the full size of the urethra, judging from the degree of pressure which it was necessary to use to make it enter the meatus. I kept this man in the hospital for fully five weeks after the wound was healed, and for more than a fortnight before his dismissal no catheter was passed, in order to ascertain, as far as possible, in that short space of time, whether any contraction had recommenced Accordingly, on the 3rd May, the day he left the house, No. 12 was again inserted, and with the same ease as formerly.

Like a great many hospital patients, this man has been lost sight of, and of course I know not in what condition his urethra is at present. But this at all events is certain, that, from the date of the operation he might have been dismissed from the infirmary in ten days, with the wound in the perineum healed —with the power of voiding his urine in a large stream —and with No. 12 catheter passing into his bladder with the utmost ease.

The third case is that of James Shortt, æt. 54, who was admitted on the 20th August. His stricture, he says, is of fourteen years' duration, and he has been under treatment for it in Ireland, in Dundee, and in Edinburgh; in fact, he was a patient some twelve months ago in the hospital here under Dr. Maclagan. When he was admitted under Dr. Maclagan's care, the stricture was very tight, and no instrument larger than No. 1 could be passed, and that was got through only after several trials. He was in the surgical wards for a considerable period, and was ultimately sent to the medical department of the house for an attack of acute rheumatism. By this time, however, the stricture was so far overcome as to admit No. 9, and he was told, upon leaving the medical wards, to go back to Dr. Maclagan as an out-patient, and have instruments regularly passed. This he neglected to do, and his stricture gradually relapsed to its former bad condition. For a fortnight after his admission no progress was made in getting through the stricture, from his having requested and obtained permission to leave the hospital on several occasions to do duty with the local pensioners—he being an old soldier—which corps happened to be on service at the time, so that he was not a regular patient until the beginning of September. On the 5th I passed No. 2, and tied it in the bladder, but in the course of an hour or so he withdrew it; on the 7th No. 3 was passed, and retained until the 8th; and on the 9th, after chloroform was given, Mr. Syme's operation was begun in the same manner as in Tucker's case, but I found the stricture so far back in the membranous portion as to prevent me cutting from behind forwards. Accordingly, having pushed the knife into the staff anterior to the stricture, I inserted the forefinger of the left hand into the rectum, and steadied the staff by pressing upon it from the bowel upwards, while with a gentle sawing motion I divided the stricture from before backwards. The staff was then removed, and No. 7 catheter passed into the bladder, but not, I must confess, without some difficulty, from the point of the instrument getting out of the urethra and catching upon the posterior part of the incision. No hemorrhage followed the operation. For the first twenty-four hours the patient suffered considerably from pain in the urethra, caused by the presence of the catheter,—this, however, was so much relieved by camphor mixture and morphia, that next day he made very little complaint. No. 7 was then removed, and Nos. 9 and 10 introduced. On the 16th No. 12 was put into the bladder; and again on the 21st and 24th, by which time the wound in the perineum was healed—no urine having flowed from it since the 14th, and at this date, the 26th, he is quite well and able to leave the hospital.

The above is the result of the three cases in which I have adopted Mr. Syme's operation; and although much more difficulty was experienced in Shortt's case than in either of the other two, yet so convinced am I of the advantage gained by it in diminishing the length of time necessary for the cure, that I intend to adopt it in those cases of bad stricture which may happen to come under my care.—*Monthly Journal of Med. Science, Nov.* 1850, *p.* 398.

85.—*Inveterate Stricture of the Urethra treated without Operation.* Under the care of Mr. Holt, at Westminster Hospital.—[Without entering into the merits of the various methods of treatment in obstinate cases of stricture of the urethra, there can be no doubt that gradual dilatation is one of very great value. Edward G., aged 50, had suffered three attacks of gonorrhœa. Premonitory symptoms of stricture came on, which, being neglected, after committing an excess, he was attacked with retention of urine, when a No. 3 catheter was passed with difficulty, but with immediate relief. On still further neglecting himself, a No. 2 was scarcely admitted; and for the twelve years previous to his admission at the hospital, the urine had passed guttatim, "continually running from him, saturating his clothes, and filling his boots."]

Upon admission, Mr. Holt desired that the bowels might be cleared with castor oil previous to the introduction of the catheter; this was attended to, and a No. 1 catheter was attempted, but it was found impossible to pass it more than half an inch from the meatus; it there entered a constricted portion, but was so firmly grasped as to defy all attempts at progression. The catheter was allowed to remain for half an hour. Three days afterwards the instrument was again introduced, but only proceeded as far as before; yet, by the most persevering efforts, at the expiration of one month, a No. 1, catheter entered the bladder, and was allowed to remain. The size of the catheters were gradually increased until a No. 8 could be passed with facility; it was then withdrawn for an hour, and reintroduced on the next day. The instrument was allowed to remain out two hours, and on the third, three hours, so that in a few days, it was only necessary to pass the same size once a day. After a further expiration it was passed every other day, and at the end of a fortnight increased to No. 9. This was shortly exchanged for No. 10, and the man now passes his urine as freely as ever; is not compelled to get up in the night, or empty the bladder more than three or four times a day. The sediment in the urine has entirely subsided, and he is rapidly gaining flesh and strength. Previous to his affection of the urethra he weighed fourteen stone six pounds, but on admission had decreased to ten stone four pounds.

Mr. Holt considers that in the treatment of this case no operative procedure would have been justifiable, the stricture being of a cartilaginous character, and extending from about half an inch from the meatus to the neck of the bladder. Mr. Holt has never had a case (although a great number come under his care) where it has been necessary to perform any other operation than the gradual introduction and increase in the size of the catheter or sound. He believes that when patience and the most gentle means are had recourse to in the treatment of stricture, no case is impassable, or will not yield to the treatment by dilatation; and that the disease will not occur any more frequently (if so frequently) as when operative procedure is employed. The most Mr. Holt considers it necessary to inculcate is the re-introduction of the catheter once or twice a year, and this is more as a precaution, and to satisfy the surgeon that the canal remains patulous.

[In the second case the patient had laboured under stricture for twenty-six years. He was fifty-two years old, and had had three attacks of gonorrhœa. Neglecting the symptoms of stricture, he delayed seeking professional assistance until complete retention had taken place. It being impossible to introduce the catheter, warm baths and opium were resorted to, with apparent relief, a little urine being passed; but the next day the urethra gave way, and infiltration of the urine into the cellular tissue of the surrounding parts took place, for which the usual treatment was adopted. Having been sent into the country for a time, he presented himself improved in health, but with two fistulous openings in the perineum, and the same impediment to the passage of the urine along the urethra.]

The same preparatory treatment was adopted in this case as in the former, and at the expiration of two days an attempt was made to introduce a No. 1 catheter. It entered the stricture but could not he passed into the bladder, and Mr. Holt was content, upon a first trial, with having brought the point of the instrument within the orifice of the stricture, the urethra being of the most irritable kind. No further interference was had recourse to until the expiration of three days, when the same instrument was again introduced with somewhat better success. Upon a third trial it passed the first obstruction, but became entangled at a second. This latter was, however, after three weeks' perseverance, (only the most gentle means being used,) overcome, and the instrument glided into the bladder. A sound, instead of a catheter, having been introduced, it was necessary that it should be withdrawn, and as it was not desirable to create too much irritation by a second passage of the instrument, no further attempt was made for three days, when after considerable difficulty a gum-elastic catheter was introduced, and secured in the usual manner.

The difficulty had principally arisen in consequence of the flexibility of the instrument employed, it not being sufficiently firm to be accurately guided along the canal. The catheter was allowed to remain two days, and having become perfectly loose, was removed, for the purpose of introducing a larger one; but in this, as happened in some other cases, the larger instrument would not pass, and sooner than create undue irritation, Mr. Holt preferred leaving the catheter out, and proceeding in the ordinary way, by gradual dilatation, beginning at the lowest number. The same sized instrument was again introduced, after a short interval; and about six weeks after the first introduction into the bladder, No. 8 passed with facility. The fistulous openings are rapidly healing, the man's appearance and general health are materially improved, and he passes his urine in a good stream.

Mr. Holt, in some remarks which he made on these cases, stated, that with regard to Mr. Syme's advocacy of dividing a stricture, through which a catheter can be passed, he considered that in 999 cases out of 1000 this operation is not only unnecessary, but perfectly unjustifiable. Mr. Holt has never seen or had a case under his own care, through which he could not get an instrument; and having passed it once fully through, he could proceed, with occasional interruptions, until a sound, the natural calibre of the urethra, was easily admitted into that canal. Mr. Holt thinks that if patients and surgeons will not have the *patience* to overcome the manifold difficulties in the treatment of stricture, the latter will of course operate; but as he considers the same end may be obtained by much less objectionable means, he never would advocate an operation that must necessarily be attended with considerable danger. Mr. Holt thinks Mr. Syme's statements quite correct, with regard to the case mentioned by that gentleman, where a catheter was retained in the bladder, until No. 8 could be passed with facility, and then withdrawn, the patient in a few days being as bad as he was before; but if Mr. Syme had gradually left off the introduction of the catheter, as Mr. Holt did in the first of the above-mentioned cases, he (Mr. Holt) would venture to assert that no such result would have taken place, but that the patient would, after three weeks or a month, have been enabled to have a larger size passed instead of recurring to his original condition.—*Lancet*, *August* 17, 1850, *p*. 219.

86.—*Retention of Urine—Puncture of the Bladder through the Rectum.* Under the care of Mr. Gay, at the Royal Free Hospital.—There are some points of practice whereon surgeons are not quite agreed, and the management of obstinate cases of retention of urine is one upon which opinions differ in a very marked degree; principally as regards the time when an artificial outlet should be made for the escape of the urine, and the portion of the urinary organs which should be punctured for that purpose. Every surgeon will, of course, endeavour by leeching, warm baths, and aperients, to obtain a natural evacuation of urine, or at least facilitate the passage of a catheter; and we had an opportunity of reporting a case under the care of Mr. Tatum of St. George's hospital, where efforts of this kind were crowned with success. (See 'The Lancet,' June 22, 1850, p. 756.) But when distention becomes dangerous, the question arises whether we should puncture either the membranous portion of the urethra behind the stricture (Sir A. Cooper), or make a free opening in the perineum directly over the obstructed part (Liston), or perforate the bladder through the rectum. Mr. Gay advocates the latter practice, and has lately been very successful in a case of retention of urine, of which we shall attempt to give an outline.

A labourer, forty-one years of age, was brought into the hospital on the 2nd of May, labouring under very distressing symptoms of retention of urine. The patient has been subject to stricture for many years; bougies have been passed at various periods; but the stream of urine had become very small, and occasionally degenerated into a dribble. On the night immediately preceding his admission, he found himself, after a debauch, unable to pass any urine; but as he was intoxicated, he went to bed, and slept until the morning. On awaking, the retention continued, and he was brought to the hospital.

The bladder, as may be easily imagined, was much distended, the patient tormented with a constant desire to evacuate it; the tongue thickly furred, the pulse quick, the thirst extreme, and the heat of skin very great. A brisk purgative was at once administered, leeches applied to the perineum, and the patient placed in a warm bath. Mr. Jackson, the house surgeon, then tried to pass a catheter, but could not succeed, even with the smallest size. After the bowels had been well opened, a good dose of opium was given, and bougies and elastic catheters tried again, but without effect. At one o'clock in the afternoon the bladder had become much more distended; and no prospect of immediate relief by the usual remedies being anticipated, Mr. Gay was sent for.

After examining the patient, and conferring with his colleague, Mr. Wakley, Mr. Gay determined that no time should be lost, and at once punctured the bladder from the rectum. A large quantity of urine passed, the canula was withdrawn, and the patient instantly dropped off into a sound sleep. An antimonial saline draught, with tincture of opium, was ordered to be given every four hours. In the evening the patient expressed a desire to pass his urine, and he did so, but with his ordinary amount of difficulty. He continued to evacuate his bladder at intervals during the night, and from that time his recovery was so rapid, that four days after admission, he left the hospital to resume his work, and was desired to attend, as an out-patient, to have the narrowed canal of the urethra gradually dilated.

This is a type of the majority of cases of retention of urine which apply at the hospital for relief; and Mr. Gay is strongly of opinion that similar treatment, instead of that which is too often pursued—viz., cutting down on the seat of the stricture, -would be attended almost invariably with similar success. The condition of the urethra, in cases like the preceding, is of a temporary character, and as it is brought about, with few exceptions, by excesses, it is to be remedied by those means which are calculated to reduce the inflammatory action which is generally combined with spasm. The time required for overcoming this state of urethra is, however, too long, while the bladder remains unrelieved, and the question then presents itself—Which is the simplest and least dangerous method of evacuating the urine?

Mr. Gay stated that, notwithstanding the objections which have been brought against the operation of puncturing through the rectum, he had not met, in his experience, any unfavourable results from this practice. He had operated on persons of different ages, and on one person in particular, under the most unpromising circumstances, and nevertheless, not a single bad symptom had ever followed. Mr. Gay believed that such had likewise been the fact in the practice of other surgeons. He therefore unhesitatingly recommended this method as being vastly superior to incisions into the perineum.

Should the stricture be of such an obstinate character that the relief which was obtained in the preceding case does not immediately follow, and the bladder again become distended, Mr. Gay would prefer (especially if there were the slightest prospect of effecting a passage by the use of bougies and other treatment) puncturing the bladder again and again, to cutting down on the stricture; and he is convinced that there are very few strictures, in the condition alluded to, which do not admit of cure by a well directed course of treatment and use of of instruments.

The dangers attending the other modes of relieving the bladder are very great, and the fatality frightful. Such operations on individuals whose constitutions have been impaired by irregularities and excesses of all kinds (and these are the very persons who are generally subjected to this treatment) are ill borne, and should if possible be avoided. Mr. Gay believes the time will come, and that shortly, when the perineal section will be very rarely performed, and the puncture from the rectum be, so far as operations are concerned, the *rule* of practice.

In the case before us, no urine passed per rectum after the withdrawal of the canula, nor has such a circumstance occurred in any case which Mr Gay has seen. The puncture of the trocar does not produce any loss of substance, but three valves are formed on the recto-vesical septum which close the aperture when the instrument is withdrawn.—*Lancet, July* 13, 1850, *p.* 59.

87.—*New Method of Relieving Retention of Urine without the Use of the Catheter.* By M. J. J. Cazenave.—[The following remarks which have appeared in ‘L'Union Médicale,’ are deserving of the attention of the profession.]

When called to a patient having retention, complete or incomplete, M. Cazenave in the first place directs the large intestines to be cleared out by an enema. When this has returned a second is administered, but consisting solely of a quart of cold water. Absolute rest on the bed is enjoined; while cloths dipped in cold water, or, better still, bladders of ice-cold water, or pounded ice, are applied to the anus, perineum, thighs, and hypogastrium. If the patient do not void his urine in the course of half an hour, or void it only very scantily, he is placed at the edge of the bed, which is properly guarded, and a stream of cold water is poured on the region of the bladder during from twenty to twenty-five minutes. After the lapse of this time another enema of cold water, and small smooth fragments of ice, are introduced into the rectum, the cold applications to the external parts being at the same time continued. The cases in which this mode of treatment is found applicable are those in which the retention proceeds from acute inflammation or spasm. - *Med. Gazette, Aug.* 1850, *p.* 242.

88.—*Case of Retention of Urine.* Under the care of Mr. TATUM, at St. George's Hospital.—Various means of relief are at the surgeon's command in cases of retention of urine: among these puncturing the bladder where no catheter can be passed must frequently be resorted to; and the nicety of the discrimination seems to consist in determining how long the attempts at passing catheters may be kept up, and how far opium and warm baths may be carried. Among our hospital surgeons, there are some who hold that extreme measures are *never* necessary, and that the judicious administration of opium, rest, and slight depletion, will always succeed in conquering the obstacle. Though we are not aware that Mr. Tatum holds exclusive opinions on this subject, we are happy to bring forward the following case, which will very satisfactorily illustrate what management, patience, opium, and warm baths can effect.

A strong-built, healthy-looking policeman, of dark complexion, was admitted, under the care of Mr. Tatum, on the 23rd of April, 1850, with very distressing symptoms of retention of urine. The only particulars that could be gathered from him were, that he had had gonorrhœa twice; two years had elapsed since he was cured of the last attack, and six months after that event he had begun to experience the inconvenience of stricture. These symptoms were, however, removed by the use of bougies, and the urethra regained its former calibre. Since that time, however, (this circumstance showing well how strictured portions of the urethra will again and again retract after dilatation,) he had had two severe attacks of retention, one of which was overcome by opium and the warm bath, and the other by the introduction of an instrument. The day before admission, after being on duty all night, the patient had again obstinate retention; the surgeon to the police force tried to pass a catheter, but failed, both before and after the warm bath.

Under these circumstances, the man was admitted into the hospital complaining of very severe pain; he was covered with profuse perspiration; the countenance was anxious; the pulse small and frequent; the skin hot; the breath and perspired fluid emitted a urinous smell, and the hypogastric region was very tense and rather tender.

The house-surgeon tried to pass a small silver catheter without success, and the urethra bled a little. The patient was immediately put into a warm bath, a soothing enema was soon afterwards administered to him, and the catheter tried again, but to no effect. Forty minims of tincture of opium were next given, and the warm bath repeated.

Mr. Tatum saw the patient about four hours after admission, and now tried to pass a flexible catheter; this having failed, it was replaced by a silver one of the same size, which Mr. Tatum succeeded, after a patient and cautious manipulation, to pass into the bladder. About a pint and a half of urine was drawn off and the catheter left in the bladder, the house-surgeon being desired to complete the evacuation of the bladder in about an hour's time. The catheter was plugged and tied in the usual manner, and appeared to be quite secure, but the patient, now much relieved, having dropped off to sleep, the instrument slipped out of the bladder (or the patient drew it out) and towards night it was found impossible to re-introduce it, though the poor man suffered great pain from retention.

He was now ordered a saline draught, with ten minims of tincture of opium every sixth hour, but in spite of a large dose of laudanum, and a warm bath during the night, no water had been passed up to seven o'clock, A.M., the next day, when the patient presented the following symptoms: —Great pain, testicles violently drawn up; penis semi-erect, profuse perspiration, with a urinous smell; stinging pain about the perineum, and in the loins. At this time, the spasm partly relaxed, and the urine began to flow guttatim, and by the middle of the day, about a pint and a half was thus voided; the testicles resumed their natural state, but the bladder remained considerably distended; the tongue dry; the skin hot, &c. Mr. Tatum now tried to pass a silver catheter, a little hemorrhage ensued, but the instrument could not reach the bladder. The patient was then prescribed five grains of calomel, to be followed by a purgative draught, and eight leeches were applied to the perineum. The leeches abstracted a proper amount of blood, and the patient was then again placed in the warm bath, during which time a few drops of urine came away, but the appearance of the man was anxious and depressed; the pain in the region of the bladder and urethra agonizing; and the moment had come when puncturing the distended receptacle might fairly be proposed. Mr. Tatum gave, however, one more trial to opium and the warm bath; and at ten o'clock, the second day after admission, the urine passed a little more freely in the bath. After this, the symptoms became again as obstinate as before; another forty minims of laudanum and a warm hip-bath were resorted to, and Mr. Tatum at length succeeded in passing a small silver catheter into the bladder. About one pint and a quarter of urine was drawn off, the catheter plugged, and well secured in the urethra by means of tape, and orders left for the remaining portion of urine to be evacuated in about two hours.

The patient felt greatly relieved, and slept very well that night, though a little pain was experienced at the extremity of the glans, whilst the urine was being drawn off. Three days after Mr. Tatum had succeeded in passing a catheter, the patient, who had had several doses of laudanum, and warm baths, during the interval, began, when the instrument was removed, to pass his water in a small stream; and for the next thirteen days the stream became gradually larger, as a rising scale of catheters, up to No. 7, were successively introduced. Twenty-two days after admission the patient was discharged, passing his urine with ease, though the stricture still grasped No. 7, and he was desired to come to the hospital once or twice a week, to have the urethra dilated.

Now from what occurs commonly in these cases, it is but too apparent, that in spite of the skill and patience with which the treatment was here conducted, a recurrence of the same symptoms. upon the slightest irregularity, is extremely likely to happen; and such cases show, in a remarkable manner, how temporary is the benefit which is obtained by dilatation; the contractile nature of the stricture always tends to re-establish the coarctation of the canal, and we are thus involuntary driven to look for means of permanent cure. It should likewise be noticed, that the men employed in the police force, by being very often exposed to wet and cold during the night, are more obnoxious than any other class of individuals to attacks of this nature; and it would almost seem proper to adopt a peculiar line of therapeutics with those very useful members of the community. They should be warned by their medical attendants, and prophylactic measures enforced among them.—*Lancet, July* 22, 1850, *p.* 756.

89.—*Fungus of the Testicle.* By Dr. W. Senhouse Kirkes, and Holmes Coote, Esq., at St. Bartholomew's Hospital.—[Without dwelling upon the morbid changes occurring in the testicle, so accurately described by Mr. Lawrence about forty years ago, we proceed at once to the consideration of this affection.]

The commencement of the disease, asssigned generally to a blow, is very frequently, perhaps always, associated with the deposit of tuberculous matter, which, infiltrated amongst the tubules, acts as a source of local irritation, and produces inflammatory swelling. The abscess which ensues should be left to burst, and should not be punctured, unless, indeed, as is rarely the case, it excites greater pain than the patient can bear; as a rule, these abscesses do far better when allowed to pursue their own course, than when treated surgically.

After the discharge of any morbid deposit, the inflammation ceases, and the protruding fungus slowly recedes, unless the change of structure has been so considerable as to convert the fungus into a firm fibrous mass. Pressure in general aids the return of the protrusion, but from various causes it may be necessary to shave it off with the knife. The scrotum adherent to the remains of the gland then cicatrises over the wound. This latter mode of treatment is founded upon the belief, that the testicle is permanently destroyed as a secreting organ, after the full protrusion of the fungus. Perhaps after the giving way of the tunica albuginea, there remains but little chance of its returning to a healthy state.

From some recent observations, Mr. Syme has arrived at a different conclusion to that heretofore entertained of the capability of the testicle in these cases to return to its natural state, and to resume its usual functions. "When," he observes, "the fungous growth is divided longitudinally, that is, from the base towards the circumference, it may seem to consist of two textures distinguished by their colour and arrangement. One is brown, and disposed in straight lines, radiating from the base, where they are nearly or quite close together, towards the circumference, where they are more or less apart, according to the size of the excrescence. The other is white and granular, lying in the spaces which are afforded by the diverging rays. The former is composed of tubuli seminiferi, altered in situation, but not in structure; while the latter is simply organisable lymph that has been effused into the interstices." Upon this opinion, he recommends another operation, which, though more tedious than that recommended by Mr. Lawrence, is stated to have the advantage of preserving the glandular substance of the testis. The hard ring of integument round the fungus being removed, a longitudinal incision is made through the scrotum, which is separated from the adherent parts below, and then united by suture over the fungus. The results are stated to be satisfactory.

As it is obvious that the preservation of the organ in an efficient state is an object of very great importance, it becomes incumbent upon all to examine the accuracy of the observations upon which Mr. Syme founds his opinion, and to draw as much as possible further information from other instances of the same disease. It is stated that, under the microscope, the tubuli show no signs of change of structure. That assertion, unless coupled with another, saying that the tubuli are unaltered in number, in the proportion of their constituents and in their relation to surrounding vessels, is of little value. Let us examine, under the microscope, a portion of the muscular substance of a heart affected with fatty degeneration, which has proved fatal to the patient; it rarely happens but that we can find portions of muscular fibre so healthy that they might be selected as specimens for demonstration; yet, who would infer from hence that the heart were healthy? The question now before us, with reference to the possibility of the restoration to a healthy state of the testicle affected with protrusion of the glandular substance, is one to be determined, partly by such evidence as can be relied upon from the patient himself, and partly by the result of accurate and repeated examinations in instances where the fungus has been removed.

[*Case.*—Joseph M., aged 34, upon recovering from an attack of pleurisy, found his left testicle enlarging, which, without pain, terminated in an abscess. This was opened; and two months after, there protruded from the cicatrix a vascular bleeding fungus. On admission, there was found adhesion of the scrotum to the left testicle, with an ulcerated opening and protrusion of a solid fungous mass. This fungous protrusion of the left testicle being unaltered in size, was shaved off by a scalpel.]

Examination of the Morbid Parts.—A thin layer of granulations covered a firm white fibrous mass, in which could be traced with some difficulty the remains of the tubuli seminiferi, much fewer in number than natural, soft, easily broken, and almost as pale as the surrounding structures. Under the microscope the white matter proved to be recently-formed fibrous tissue; it composed by far the greater part of the protruding mass. The tubuli were so soft that they broke under the most careful dissection. The epithelial cells were here and there arranged in their normal state; but were mostly indistinct, and mixed with a quan tity of amorphous granular matter. The whole glandular structure appeared in a condition of advanced atrophy.

The wound of the scrotum healed readily, and, under proper treatment, the opposite testicle is recovering its normal size and condition.

About a year ago I examined the testicle of a gentleman who had suffered from this disease; the protruding fungus had retired by the application of pressure; the epididymis remained hard during life. The vas deferens was hard, and adherent to the surrounding parts; the canal was patent, but much smaller than natural, and mercury forced into it caused rupture of the coats; the tubuli seminiferi were very few in number, paler than natural, and intermixed with white fibrous tissue; the epithelium appeared unchanged under the microscope; but there was no trace of secretion in the tubes, as is usually the case when the canal of the vas deferens is simply obstructed or narrowed.

September 29, 1846.—I injected and examined a testicle which had been removed for this fungous protrusion of the tubules; minute injection flowed readily into every part, and coloured the external layer of granulations; the spermatic cord was thickened; the vas deferens and the epididymis were unchanged in structure, but full two-thirds of the glandular substance was converted into a firm white mass, in which scarce a trace of tubuli seminiferi could be found: the fungus had made its way as usual through an opening in the tunica albuginea and the adherent scrotum.

I apprehend, that in these three cases the secreting power of the testicle was permanently destroyed; and no operation, undertaken with the view of preserving the organ, would have been followed by any good result.—*Medical Times, July* 6, 1850, *p*. 9.

90.—*Operation for Phymosis.* By MM. CHASSAIGNAC and VIDAL (DE CASSIS).—MM. Chassaignac and Vidal are respectively the authors of an operation for phymosis by the method of circumcision. That of M. Chassaignac consists of the introduction into the cavity of the prepuce of a blunt forceps, or any other dilating instrument with two branches, which in opening form the letter V. When the tension and flattening out of the prepuce are once accomplished, M. Chassaignac implants three needles in the base of the cutaneous triangle, which are passed through only the half of their length. They are then given to an assistant to hold, and the forceps are withdrawn. A fine strong thread is now taken, and a circular ligature made in front of the needles, which strangulates all the part of the prepuce that had been dilated. The scissors, carried perpendicularly in the furrow made by the ligature, divides at one cut integument and mucous membrane, after which the needles are passed quite through with their threads, the loops which they form between the opposite edges of the prepuce are divided, and twice as many separate ligatures as there were originally needles are thus obtained. The remainder of the operation consists in bringing the mucous membrane and the skin accurately into apposition at the edges of the wound, by means of the ligatures. The reviewer in the 'Bulletin de Thérapeutique' claims for

M. Vidal (de Cassis) the merit of having employed this process three years before it was proposed by M. Chassaignac. M. Vidal appears, however, to have since adopted another process. It consists in tracing with ink on the prepuce a line indicating the point where the section is about to be made, then grasping the prepuce with a dressing forceps, or a forceps of continuous pressure, in front of this line, the operator draws the prepuce a little forward, and having passed three needles with waxed thread through it at the line traced by the ink, divides at one cut the mucous membrane and skin. The following practical observations of this distinguished surgeon, in reference to the operation for phymosis, deserve attention:

"I have been hitherto in the habit of reuniting the mucous membrane and integument immediately after the section. But I have observed, after these instantaneous unions, ecchymoses of the sheath of the penis, thrombus of the base of the prepuce, infiltrations, and sanguineous collections, which prevented the immediate healing of the parts. I have, therefore, in regard to this operation, determined to follow the precept of Dupuytren, who allowed a certain time to elapse between the operation and the dressing. I perform the section at the commencement of my visit, during the remainder of which the wound is bathed with fresh water; and when I come again to my patient, the vessels have disgorged themselves, the wound no longer bleeds, and is in the most favourable circumstances for immediate union."—*Dublin Quarterly Journal of Med. Science, August,* 1850, *p.* 120.

DISEASES OF THE SKIN.

91.—ON FAVUS.

By Professor **Bennett**, Edinburgh.

[Professor Bennett, in his clinical reports, first proposes saying a few words with regard to eruptions of the scalp in general, because of the frequency of their occurrence, and because great confusion has been thrown over the subject by systematic writers.]

There was a period in the history of skin diseases when they were arranged in two great divisions, viz., those affecting the scalp, and those affecting the rest of the cutaneous surface. All the disorders comprehended in the first of these divisions received the name of Porrigo, a word said by some to be derived from *porrum*, on account of the scales or concretions of the scalp, resembling the layers of an onion; by others, it is derived from *porrigere*, to spread. Willan described six kinds of Porrigo, viz., P. larvalis, P. furfurans, P. scutulata, P. favosa, P. lupinosa, and P. decalvans. It is now ascertained that none of these diseases are necessarily peculiar to the scalp,—and that, although they are more or less modified by growing among, and matting together, the hairs of that region, they may also occur on other parts of the skin. There can be little doubt, however, that the employment of the term

Porrigo, as well as the corresponding word *Teigne* in France, has thrown great confusion over the subject of eruptions on the scalp. It will be well, however, as this term is still in pretty general use, to explain to you what diseases these different kinds of Porrigo really are.

Porrigo larvalis (larva, a mask) is really Impetigo, or Eczema impetiginodes of the scalp. The former is recognised by crusts more or less prominent or nodulated; the latter, by the circumstance that, in addition to these nodules, there is between them a laminated or brittle crust, spread more or less equally over the surface. They are both very common in infants and children; and, as the disease sometimes extends over the face, concealing the features, hence the term *larvalis.* A very characteristic representation of impetigo capitis, which I now show you is given in *Willan* and *Bateman,* Plate xli., erroneously called Porrigo Favosa.—See also the disease on the face. *Ibid.,* Plate xxxvii. *Alibert,* Planches 13 and 15.

Porrigo furfurans (furfur, bran) is really Pityriasis of the scalp, although Psoriasis of that region has also received the same apellation. There is also a peculiar form of Eczema or Eczema impetiginodes, in which the crust is friable, and breaks up, or crumbles into minute fragments, to which the term furfurans has been erroneously applied. The true Porrigo furfurans (Pityriasis) is well represented.—*Willan* and *Bateman*, Plate xxxviii. *Alibert,* Planches 14 and 15.

Porrigo scutulata (scutulum, a small shield).—The nature of this disease has been much disputed. By some, it is said to be Favus (Erasmus Wilson), by others a form of Herpes (Cazenave, Neligan, Burgess). The disease is described by Willan and Bateman, and more recently by Burgess, to consist of oval or rounded slightly elevated patches, covered wish furfur, and having stunted or filamentous hair projecting from the surface. It is a form of skin eruption exceedingly rare in Edinburgh. It seems to be represented, *Willan* and *Bateman,* Plate xxxix. *Willis,* (Trichosis Scutulata.)

Porrigo favosa (favus, a honeycomb) is a disease, the true nature of which has been only lately determined. It consists essentially in an exudation on the skin, in which fungi or phytaceous plants grow. Round, isolated, bright yellow crusts are formed, which when compressed together, assume an hexagonal shape. Hence the term *favosa.* It is well represented, *Willis,* (Trichosis lupinosa.) *Erasmus Wilson,* Fasciculus I. *Alibert,* Planche 17.

Porrigo lupinoso (*lupinum,* the lupine).—This is the same disease as the last. The round or oval crusts when isolated, and at an early stage, present a concavity and form, resembling that of the lupine seed—hence its name.

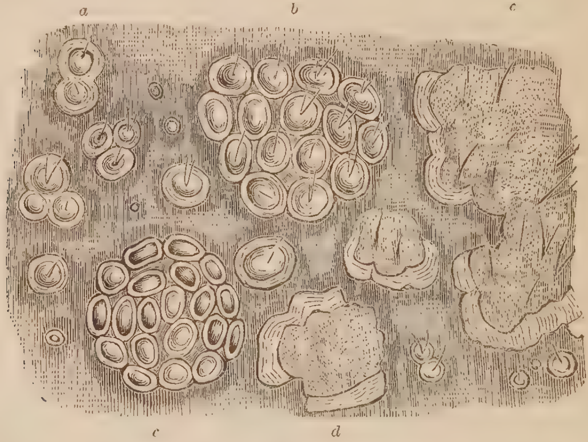
Porrigo decalvans (calvus, bald).—Baldness is so common among the aged, that it can scarcely be called a disease; but when it occurs in young persons, and is circumscribed, it constitutes the Porrigo decalvans of Willan. It issaid by Gruby to depend on a vegetable parasite growing in the hair. It is well represented, *Willan* and *Bateman,* Plate xl. *Willis,* (Trichosis Decalvans.)

From this analysis of the different kinds of the so-called Porrigo, you observe that there is nothing peculiar with regard to them. With the exception of baldness, none essentially belong to the hairy scalp. True favus is far more common on the head than elsewhere; but I have frequently seen it on various parts of the cutaneous surface, and occasionally on the cheeks or shoulders, without being on the scalp at all. I propose, then, abolishing this term Porrigo altogether, and, in future, making use of the term Eczema Impetigo, Pityriasis, Psoriasis, or Favus of the scalp, as the case may be. It is this latter disease that I now propose to describe to you more particularly.

History of Favus as a Vegetable Parasite. The demonstration by Bassi of the vegetable nature of the disease named muscardine in silkworms, which causes so great a mortality amongst those animals, opened up to pathologists a new field for observation, and led to the discovery, that certain disorders in the higher animals, and even in man himself, were connected with the growth of parasitic plants of a low type. Schönlein, of Berlin, was the first to detect them in favus crusts,—an observation confirmed by Remak, Fuchs, and Langenbeck. Gruby gave a very perfect description of these vegetations, in 1841, and made numerous researches as to their seat, origin, and mode of propagation. These were repeated by myself, and further extended in 1842. In 1845 I succeeded in inoculating the disease in the human subject. Since then they have been made the subject of further investigation, by Lebert, Remak, Robin, and numerous other inquirers.

[As to the mode of development and symptoms of favus, some describe it as commencing in a pustule, and others as in a crust, but,]

It would appear that the first morbid change is increased vascularity of the skin, accompanied with a desquamation of the cuticle; and that in a period varying from twelve to fourteen days, small spots of a bright yellow colour, like that of sulphur, may be detected. These gradually augment in size; but even at the earliest period, may be observed, with a lens, to have a central depression, through which a hair may generally be observed to pass. The crust or capsule may enlarge to about the size of a shilling, and if it be isolated, still retain its rounded form. Usually, however, its edges come in contact with other capsules, and then it loses its rounded shape, and assumes the hexagonal and honeycombed appearances described by authors. I consider, then, that the so-called porrigo lupinosa, and porrigo favosa, constituting distinct forms or varieties of some writers, are merely different stages of the same disease, and dependent upon the greater or less aggregation of the crusts. On the first appearance of the capsule, its edges are somewhat depressed below the surface of the cuticle; but as it increases in size, they become more and more elevated, and at length are very prominent. At first, also, the whole capsule appears of a homogeneous bright yellow, but when further developed, its centre assumes a whiter colour. This arises from the aggregation of the sporules of the plant, which are more abundant in this situation. As the development proceeds, this central yellow whitish mass assumes a mealy, powdery consistence, and encroaches upon the edges of the capsule, which gradually disappear, whilst its upper concave form becomes convex, as Gruby pointed out. In general an inflammatory ring is seen round the crust, which as the capsule becomes elevated above the skin, enlarges, and assumes a deeper colour, indi cative of the increased local irritation. At length the whole cracks or splits up; all regular form is lost; a dense thick crust covers the scalp; an odour, like the urine of cats or mice, is evolved; and in chronic cases, vermin deposit their eggs in the interstices, and crawl in large numbers over the surface.



a, Isolated crusts of Favus, presenting the lupine seed-like depression in different stages of growth (so-called *Porrigo lupinosa*); some are arranged in groups of two and threes. *b*, A larger group of these crusts, somewhat compressed at the sides, like a honeycomb (*Porrigo favosa*). *c*, Another group, which occurred on the shoulder of a young girl. No hairs passed through the centre of these crusts. *d*, Large isolated crusts in an advanced stage of growth, the external ring is cracked, and the friable centre is enlarged and elevated *e*, Numerous crusts aggregated together, so as to form an irregular elevated mass. Traces of the original form may be observed in the cracked rings round the margin. (Natural size)

The other local symptoms are merely those which result from the greater or less degree of irritation produced in different persons by the changes above referred to. At first, scarcely any uneasiness is felt; perhaps occasional slight itching at the part. As the disease progresses, however, the itching becomes more intolerable, and induces the patient to rub and scratch the scalp. By these means, several of the crusts are forcibly torn from their attachments, and considerable effusion of serous fluid and blood is produced. Sometimes inflammation is thus occasioned. Impetiginous pustules are frequently formed, or suppuration produced, terminating in ulceration, and the discharge of an ichorous fluid from beneath the crusts. At an advanced stage of the disease, the peculiarly offensive odour exhaled is insupportable to those who surround the individual, and the ichorous discharge, vermin, and crusts, which cover the affected parts, present a most disgusting appearance.

Although the disease most commonly attacks the hairy scalp, it may occur on the forehead, temples, cheeks, nose, chin, ears, shoulders, arms, abdomen, lumbar region, sacrum, knees, and legs. Alibert gives a plate in which it is figured in all these situations. I have myself seen it on the cheek, shoulders, back, arms, and inferior extremities, and in some of these situations I could detect no hairs perforating the capsules. —(See Fig.)

The constitutional symptoms are of the utmost importance, but, generally speaking, receive little attention from practitioners. In most of the individuals affected, who have come under my notice, the general health has been greatly deranged, and a scrofulous or cachectic constitution more or less evident. In some the *facies scrofulosa* of authors has been well marked; in others there were engorgements of the lymphatic glands of the neck; and in the only fatal case which has come under my observation, there were found tubercular depositions in the lungs, mesenteric glands, and other textures. Indeed, the generality of individuals who die labouring under favus, perish from phthisis, or other forms of tubercular disease.

By those not well accustomed to the diagnosis of skin diseases, favus has often been confounded with other eruptions of the scalp, more especially eczema and impetigo, or the combination of these diseases known as the eczema impetiginodes. In none of these eruptions, however, do the yellow crusts or scales present traces of vegetations when examined microscopically. This, therefore, furnishes the real diagnostic and pathognomic character of the disease.

That the disease is inoculable, and capable of being communicated by contagion, there can be no doubt, a result which accords with the observations of most practitioners, and with numerous recorded facts. It does not readily spread to healthy persons, and there must be either a predisposition to its existence, or the peculiar matter of favus must be kept a long time in contact with the skin.

[With regard to the pathology of the disease, Professor Bennett says:]

With the exception of Mr. Wilson, who seems carefully to have examined the favus crust, the opposition to the vegetable nature of this production, seems to have originated in very imperfect notions as to its intimate structure on the one hand, and that of certain cryptogamic plants on the other. For if long hollow filaments, with partitions at intervals, containing molecules within their cells, springing from an unorganized granular mass, and giving off towards their extremities round oval bodies, or sporules, arranged in bead-like rows, be not vegetables, what are they? The animal tissues present nothing similar, while numerous plants, long known to botanists, present the same identical structure. But not only must they be referred to the vegetable kingdom, but to a considerably elevated position among the cryptogamic plants.

With regard to the idea of Mr. Erasmus Wilson—that they are modifications of epidermic cells—it must be observed that this touches upon a great question in pathology—namely, whether particular cells have certain definite characters and modes of development, or whether one kind of cell may be modified into others. I have frequently asked myself whether accidental circumstances might not transform an epithelial or hepatic cell into that of cancer, or whether various healthy cells might not become morbid ones. Without entering into this subject at length, I need only state my opinion that, in the present state of science, every fact leads to the conclusion, that cells, once formed from a blastema, always advance according to laws originally impressed upon them. Consequently, epidermic cells, though they may be transformed into hair, horn, or other epidermic structures, cannot be changed into pus cells on the one hand, or into those resembling vegetable growths on the other. If this view be correct, it follows that we must regard the branches and sporules in the favus crust, as consisting of true vegetable parasites, and as cryptogamic plants, growing in the animal textures.

M. Cazenave, in his recent work, supports the opinion, that the sebaceous glands are the primary seat of favus. He states that the disease *always* forms round a hair, and is never produced if the follicle be obliterated, or on a cicatrized surface devoid of hair. He considers the matter of favus to be a peculiar secretion, or a modification of the normal matter secreted by the sebaceous glands, analogous to what exists in *acne sebacea.*

Mr. Erichsen considered, "That the matter of favus is a modification of tubercle—that it is a tubercular disease of the skin. By tubercular I do not mean a disease like lupus, characterised by small firm tumours, but a disease, the nature of which consists in the deposition of that *heterologous* formation called tubercle. This view of the nature of favus I have long held; and it was distinctly stated by me, when treating of the pathology of scrofula, in a work published in 1841. The favus crust, however, is not constituted alone of tubercular matter. This peculiar exudation only constitutes the soil from which the mycodermatous vegetations spring, as I shall now endeavour to show.

Gruby describes the micodermata of favus as springing from an amorphous mass, of which the periphery of the capsule is composed. This mass undoubtedly exists, and, according to my observations, is composed of a finely molecular matter, identical in structure to certain forms of tubercle, or recently coagulated exudation. The cheesy matter, for instance, so frequently found on the secreting surface of serous membranes, and in tubercular cavities and other structures in chronic cases of tuberculosis, or general tendency to tubercular deposition, presents this character. Every pathologist who has minutely examined tubercle, recognises a granular form, in which there is no trace of nucleus or cell, and which, therefore, we are warranted in considering as unorganised. I have myself repeatedly examined this tubercular matter, and been unable to detect any difference between it and the mass in which the vegetations of favus appear to grow. Chemical analysis of this form of tubercle demonstrates it to be composed principally of albumen, with a minute proportion of earthy salts; sometimes there is combined with it a small quantity of fibrin or gelatine. If this general result be compared with the analysis, by Thenard, of favus matter, the identity between it and tubercle must appear highly probable. He found in 100 parts coagulated albumen, 70; gelatine, 17; phosphate of lime, 5; water and loss, 8 parts. Thus the evidence furnished by morphology and chemistry, agree in determining the molecular matter found in the crusts of favus and of tubercle to be analogous.

All the facts which recent researches have brought to light, only tend to confirm the conclusion which I arrived at on this subject in 1842, as expressed in the following passage :—

"In man all the vegetations yet discovered have been found connected with the matter effused into the textures in scrofulous constitutions. The fungi found by myself, for instance, growing in the tuberculous cavities of the lungs, and those discovered by Schönlein, and described by Gruby, constituting scrofulous eruptions on the skin, grew on a finely granular amorphous mass, which presented no evidence of organisation. Chemical researches have shown, that this form of tubercular matter is principally composed of albumen,—which explains the large proportion of this animal principle present in the crust of the porrigo or tinea favosa, according to the analysis given by Alibert. The fungi found by M. Eudes Deslongchamps growing on the membranous lining of the air-passages in an eider-duck, sprung from an 'albuminous layer,' 'forming the soil on which they grew.' The mould, or mucor, discovered by Owen growing in the lungs of the flamingo, occupied the same situation as those observed by myself in the lungs of man,—viz., the lining membrane of tubercular cavities. The fungi found by MM. Rousseau and Serrurier in the parroquet grew on a species of false membrane. What the nature of this membrane was is not stated, but it is distinctly mentioned that the animal died of laryngeal and pulmonary phthisis. In pigeons, also, the same authors describe it as commonly induced by exposure to cold and moisture, circumstances well known to be the most common cause of scrofula and of tubercular depositions.

I believe, therefore, that the pathology of favus is best understood by considering it essentially to be a form of anormal nutrition, with exudation of a matter analogous to, if not identical with, that of tubercle, which constitutes a soil for the germination of cryptogamic plants, the presence of which is pathognomonic of the disease. This matter may be found primarily or secondarily. That is, there may be want of vital power from the first, as occurs in scrofulous cases, or there may have been production of cell forms, such as those of pus or epidermis, which, when disintegrated and reduced to a like molecular and granular material secondarily, constitute the necessary ground from which the parasite derives its nourishment, and in which it grows.

[Dr. Bennett remarks on the treatment of this disease, that almost every kind has been had recourse to in order to remove this disagreeable and intractable disease; and no doubt cases have been cured by all or any of the methods recommended]

I have endeavoured to show, however, that in many cases it is a constitutional disease, and dependent upon the causes which induce scrofulous diseases in general. The treatment, therefore, in such ought to be constitutional; and directed to remove the tendency to tubercular exudation, on which the malady depends. No doubt, however, a local treatment in this, as in all disorders which are at the same time general and local, is of the utmost service.

I consider, then, that the chief indications of treatment are, 1st, To remove the constitutional derangement; and 2ndly, To employ such topical applications as tend to prevent the development of vegetable life. This line of practice may be thought similar to that recommended long ago by Lorry, who advises, 1st, A modification of the fluids and solids of the economy by a general treatment; 2dly, A vigorous attack upon the local disease by topical applications, capable of removing the crusts, causing the skin to suppurate deeply, and substituting a solid cicatrix for the morbid ulceration of the hairy scalp. For the most part, however, the general treatment of physicians has been confined to diluent drinks, blood-letting, purging, and remedies which depress the vital powers, whereas it must be evident, that if the views of its pathology I have brought forward be correct, and it is in its nature allied to tubercular affections, a treatment exactly opposite ought to be pursued. The development of vegetable life may also be prevented by the application of much milder remedies than the escharotics, or irritating ointments usually employed.

The treatment of scrofula will be fully entered upon by me in a subsequent lecture. Suffice it to say at present, that I shall endeavour to show how this peculiar cachexia is caused and kept up by some fault in the digestive process; that the blood is secondarily affected, and its albuminous constituents proportionally increased; that the albumen at length becomes effused into the different structures of the economy, causing the various forms of tubercular disease; and, lastly, as the albumen in the blood becomes excessive, and its effusion into the textures increases, the fatty constituents of the frame diminish. It will be shown, by an appeal to numerous facts, that under such circumstances the internal and external exhibition of cod-liver oil has been attended with the most marked advantage, and often been made the means of cure when all other remedies have failed. The action of the oil appears to be the same in favus as in other forms of scrofulous disease, and its use should be combined with appropriate diet and exercise, and with reference to the same indications and contra-indications. As it is my intention to describe these minutely on some other occasion, it is unnecessary for me to allude to them at present.

The local treatment I have employed for several years, is directed, in conformity with the pathological views previously detailed, to the exclusion of atmospheric air, so as to prevent vegetable growth. For this purpose, I direct, in the first instance, that the affected scalp should be poulticed for several days, until the favus crusts are thoroughly softened, and fall off. Then the head is to be carefully shaved, after which it will be found to present a shining clear surface. Lastly, cod-liver oil should be applied with a soft brush, or dossil of lint, over the affected surface morning and night, and the head covered with an oil-silk cap to prevent evaporation, and further exclude the atmospheric air. Every now and then, as the oil accumulates and becomes inspissated, it should be removed by gently washing it with soft soap and water. It is very possible common lard would do as well as cod-liver oil; and I observe that Dr. Girot has lately recommended olive oil as a local application.

I have found the average duration of this treatment to be six weeks, which contrasts very favourably with the results of M. M. Mahon's practice at the Hôpital, St. Louis. Some cases seem to be incurable, and these are most frequent among adults, but even in them, so long as the scalp is kept moist with oil, and excluded from the air, the eruption will not return. In young subjects, in whom general as well as local treatment is admissible, and in whom a scrofulous disposition is manifest, the prognosis is more favourable, and the disease may be permanently eradicated. Whenever favus is recent and of limited extent, it may at once be destroyed by cauterization with nitrate of silver.—*Monthly Journal of Med. Science, July*, 1850, *p.* 43.

92.—*On Eczema.* By Professor Bennett, Edinburgh.—[Dr. Bennett records two cases, one in a painter, aged 40, who had had an eruption twenty years. The other in the person of a widow, aged 26, which appeared to have arisen from an injury to her elbow from a fall five months before admission. Professor Bennett observes,]

Eczema essentially consists of diffuse crops of minute vesicles, seated on an inflamed red surface, which, on breaking, yield a thin serous looking fluid that coagulates on the surface, so as to form a flat laminated scab. There is almost always intense itching, or a feeling of burning or smarting, often very painful; symptoms most intense in acute, but also present in chronic, cases. In these last, the skin often assumes an unusual degree of induration, a more or less deep red colour, and presents cracks and fissures, giving rise to grest uneasiness. The first case above detailed is one of this kind, and presented all the characters of extreme obstinacy. The second case was more acute. Both were treated with local applications only, and with the best results.

The application I employ is a solution of ʒ ij. of the subcarbonate of soda, in a pint and a half of water. But an alkaline lotion in itself, applied from time to time, as is most commonly done, will not prove of any great service. I have found it necessary to place lint, saturated with the solution, over the affected parts, and to cover the whole with oil-silk, in order to prevent evaporation. Keeping the surface moist seems to me a necessary part of the treatment. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean, and prevent the accumulation of those scabs and crusts which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, merge into that of the healthy skin, and finally the whole surface assumes its normal character.

I am quite satisfied that this local treatment is far superior to the stimulating ointments and lotions, which are so frequently used one after the other, in the hope of overcoming these obstinate forms of eruption. Many of them, indeed, only seem to augment the disorder. In these cases there is an increased exudation from the skin, not only of sebaceous but of purulent matters. Alkalies, we know, have the property of dissolving these, and acting as a calmative and emollient to the irritated part. Their constant action on the skin, therefore, in the manner I have described, may easily be imagined to be productive of benefit. But if, as is usually done, alkaline lotions and baths are only employed from time to time, they remove the sebaceous matter, leave the skin dry and harsh, and thus these applications often irritate. Keeping the surface covered with moist lint, on the other hand, while it serves to protect, removes the results of exudation, and prevents incrustation.

I do not wish it to be understood that this is the *only* local application which can be useful in these cases, but that I have found it more extensively applicable, and more uniformly serviceable than any other.—*Monthly Journal of Medical Science, July,* 1850, *p.* 59.

93.—*On Psoriasis.* By Prof. Bennett, Edinburgh.—[Of this disease Dr. Bennett describes three cases. The first occurring in a baker, aged 36, over the inferior extremities and various parts of the trunk. The second in a washerwoman, aged 36 also, which appeared subsequently to an attack of cholera; and the third in a servant, aged 15, in whom the eruption had appeared eight months previous to admission, first on the inferior and then on the superior extremities.]

So-called lepra vulgaris, psoriasis diffusa, psoriasis gyrata, psoriasis guttata, and psoriasis inveterata, are one disease, more or less chronic. In the works of systematic writers, the varieties of this affection are multiplied indefinitely, even to giving minute separate descriptions of it, as observed on the eyelids, lips, prepuce, palms of the hands, nails, &c. &c. &c. The sooner you disembarrass yourself of all such useless distinctions the better.

The real nature of psoriasis is altogether unknown. I am not aware that any researches have been made as to the alterations which the structure of the skin undergoes in the disease. All that can be said is, that, as we see a considerable redness, there can be little doubt of there being a congested state of the vessels, combined with increased amount and desquamation of the cuticle. There is no exudation, properly so called, but an increased growth, or an hypertrophy of the epidermis.

Observation has shown that this, like many other skin diseases, is sometimes connected with a deranged state of the digestive organs, sometimes with a constitutional disorder, very little understood, whilst in others (but these are comparatively few) it is more or less dependent on local causes. A judicious treatment of this disease will materially depend on a correct appreciation of one or the other of these circumstances, or of their various combinations.

The constitutional treatment which I have found to be most efficient in cases of psoriasis, is the administration of equal parts of Fowler's solution, and of tincture of cantharides, in doses commencing with ten drops, gradually increased to fifteen or twenty. The effects of this medicine must, of course, be carefully watched. Of all the local applications, most dermatologists are agreed that the best is pitch ointment. It seems to exert a specific action on the skin in these cases, and of its value there can be no question. You will occasionally find, however, that it causes considerable irritation, in which case it should be diluted with an equal part of lard. In Case I. this occurred, and simple ointment only was employed, which, combined with the internal use of arsenic, was sufficient to produce a cure. In Case II., the arsenical and cantharides drops, combined with pitch locally, brought about recovery; whilst, in Case III., the same result was obtained by the local application alone. Such is a fair specimen of what you may expect in the treatment of this disease. Some cases getting well under arsenic alone; others, under pitch ointment alone; whilst a third case will require the action of both.—*Monthly Journal of Med. Science, July,* 1850, *p.* 60.

94.—ON THE TREATMENT OF PSORIASIS AND LEPRA-VULGARIS.

By M. Emery.

The following is a sort of statistical balance-sheet drawn up by M. Emery as the result of his experience in the St. Louis Hospital of Paris:—

He states that, after having failed to obtain any good amount of success from following systematic plans of treatment,—such as the purgatives of Hamilton, the blood-lettings of Duffin, Wallace, and Graves, and the lunar caustic applications of Alibert,—he adopted the plan of Biett, who had introduced the treatment by arsenical preparations, tincture of cantharides, and frictions with iodide of sulphur. Here are the results of these methods in his hands:—"I have treated with different preparations of arsenic 140 cases of psoriasis, or lepra vulgaris, and have kept notes of them. Of these only 38 have gone out apparently cured, after two, four, six, eight, and even fifteen months of treatment. Within six months, six of these (three women and two men) had returned to my wards. Within eighteen months, I had re-admitted twenty-two. I have never again seen the other sixteen. Of the 102 other cases, forty went out covered with darkish spots, having scales of psoriasis alongside of them, or even upon them, but in small quantity. Before the end of a year, I saw twenty-four of these again. The shortest treatment had been three months, and in eighteen of them (eleven men and seven women), the duration had not been less than a whole year. The remaining sixty-two had so often suffered from various accidental ailments, produced by the remedies, that after eight or nine months of ineffectual treatment, I submitted forty of them to tar ointment frictions for two or three months, when they went out cured. On twelve of them I had tried tincture of cantharides, but only cured one solitary case,—the other eleven went out of hospital without much improvement, after remaining in a year. Lastly, of the remaining ten I treated two with ointment of napthaline (one part to fifteen of axunge). These were cured in six weeks. I treated eight with proto-iodide of mercury ointment (one part to sixteen); an ounce of this being used daily, which, in four, produced sharp itching, and in two swelling of the gums, with salivation, after a month of treatment. They seemed radically cured in two months and a-half." In all these cases the various forms of treatment were accompanied by simple or sulphureous baths, or with the vapour bath or vapour douche. The preparation of arsenic which he found to answer best was Fowler's solution, in doses of from five to eighteen drops The arsenical plan is not without a certain amount of danger, and sometimes it has to be suspended six or eight times in the course of the treatment; but M. Emery has no hesitation in proclaiming it to be the best of all the internal remedies.

He then refers to external remedies, and after noticing a great many which had failed in his hands, he mentions his having himself introduced, in 1836, tar ointment in large proportion (a fourth or a third of a part of tar to two or three parts of axunge), against which no graver objections have been offered by the many practitioners who have adopted it, than its soiling the linen and being palpable to the senses. It cures more rapidly, and as surely as any other remedy. Even in the largest quantities it does not injure the health, and only produces occasionally a few superficial pustules which heal rapidly. He had treated by this means from fifteen to eighteen hundred cases, had cured more than five-sixths of them, and never saw any unpleasant consequences attend its use. He at first employed along with it calomel and sulphuric acid internally, and sulphureous baths externally; but discontinued them, not only because they appeared to be useless, but because they seemed even to retard the cure. He subsequently contented himself with a few simple baths at long intervals. He thinks patients treated in this way not so liable to relapses as those treated by internal remedies alone, and especially by arsenic. Upon a suggestion of M. Cazenave, however, he tried a combination of the external treatment by tar and the internal administration of arsenic, and the following are the results:—"I have treated by this method 228 cases of psoriasis, and cured 200 in the average space of six weeks to two months. I have had very few effects of the nature of poisoning, having seldom exceeded ten drops of Fowler's solution daily. I have observed that by this treatment the disease underwent at the same time two curative processes: the scales of psoriasis are by the tar effaced from the circumference to the centre, and by the arsenic they diminish in thickness and assume a blackish grey tint. By combining these methods of treatment, these two phenomena manifest themselves at the same time."

In the cases which resisted this treatment, amounting to about a seventh of the number, M. Emery tried various mercurial preparations, but found only one to succeed,—that of M. Boivet, which is one part of proto-iodide of mercury to eight of axunge; but it has the disadvantage, when applied over large surfaces, of frequently causing salivation. Another ointment which is also of some value is that of iodide of sulphur, containing from one part in thirty-two to one in eight of lard. Psoriasis of the scalp yields to this, but it is apt to cause erysipelas, when applied over large surfaces, and especially when used of the strength of one to eight. M. Emery's conclusions, then, are,—"That arsenical preparations, and especially Fowler's solution, are the best internal remedies; that tar takes the first place as an external application; that the combination of these is the best treatment for psoriasis; that the iodide of mercury ointment is occasionally useful when judiciously applied, and next to it the iodide of sulphur."

He also tried, on the faith of an article in the 'Gazette des Hôpitaux,' about three years ago, baths containing five drachms of bichloride of mercury, which were said to cure psoriasis in eight days.

These baths were tried first in twenty-two cases (fourteen males and eight females), subsequently upon twenty others, sixteen of whom were under fourteen years of age. None were cured, while several suffered severely from the remedy. M. Gibert had also tried these baths in fifteen cases, and failed in all of them; so that the inutility of this remedy appears to be settled.

M. Emery adds, that too great reliance must not be placed upon the certainty of the cures, as relapses are numerous,—the same causes which produced the disease being apt to cause its recurrence. Dietetical and hygienic measures must also be adopted; the diet mild and not too substantial; all aliments difficult of digestion to be avoided, especially salted meats, the richer kinds of fish, and fried articles. The patients should take regular exercise, use baths frequently, and have recourse again to the treatment which had benefitted them, on the least appearance of the disease, without waiting till it has developed itself extensively.

The following is his method of applying these remedies:—"Before commencing the frictions I order a bath, and make slight frictions with the ointment the moment the patient comes out of it. This is repeated three times daily, and at the end of two or three days I increase the quantity of the ointment and the activity of the frictions. After six or seven days the patients have the ointment constantly upon them; and when the disease is of long standing I cover the larger patches of eruption with compresses spread with the tar ointment to the depth of a line. The patients take a tepid bath once or twice a-week. It is rarely necessary to suspend this treatment. There are, however, subjects with very irritable skins, in whom it produces occasionally a few impetiginous pustules or some small boils: but very often they continue their treatment in spite of these. In about ten days there is observed in the patches of psoriasis, from which the scales have separated, a whitish circle around them, which gradually extends from the circumference to the centre. This is the first indication of the decrease of the complaint, which com. monly disappears in the space of two or three months, without the patient experiencing any change in his general health. In the lepra vulgaris the centre commences to break up. The rings, which form the circular chain of eruption, separate, and then follow the same course as the psoriasis.

The Fowler's solution should be given cautiously, beginning with five drops in four ounces of liquid, which is divided into two portions. An additional drop is given each day; and if no unpleasant consequences follow, the quantity may thus be rapidly raised to twelve drops daily. If it is observed that the patches of eruption become thinner, and assume a blackish-gray colour, the dose is not increased further for fear of accident, as the above appearances indicate saturation of the system. When, on the contrary, the patient bears the remedy well, and there is no appearance of improvement, the Fowler's solution may be augmented in quantity to fifteen or sixteen drops, which it is rarely necessary to exceed.

"The skin sometimes becomes the seat of rather sharp pain, or, without being absolutely painful, becomes hot and slightly red around the patches; a few tepid baths, diluent drinks, and the diminution of the dose of arsenic, suffice to remove these slight symptoms. There are some delicate stomachs which cannot support the arsenic even in the smallest doses; and when that is fairly established we must have recourse to some other treatment. Sometimes, after continuing the above treatment for fourteen or fifteen days, there occur sense of constriction in the throat, and rather severe pain in the stomach. The whole treatment must then be suspended. These symptoms disappear in two or three days. The arsenic may then be resumed, recommencing with five drops in the day, and increasing the dose by one drop every second day. The pains in the region of the heart, which render all its pulsations painful, sometimes make it necessary to have recourse to blood-letting. The symptom demanding most attention is, undoubtedly, contraction of the extensors of the limbs. So soon as this shows itself the arsenic must be abandoned, or we run the risk of seeing this increase and become rapidly incurable.

"When the patients have been able to bear full doses of the remedy, and when the patches of eruption have given place to spots of a blackish-gray colour, the use of the arsenic may be discontinued, and the treatment limited to baths and vapour douches. The saturation is then complete, and the arsenic would not then act any farther on the system at large. These spots which announce the approaching cure, may not disappear till after some months of treatment."—*Bul. de Thérap.—Monthly Journal of Med. Science, September*, 1850, *p*. 274.

95.—*Cutaneous Cancer of the Ankle—Amputation of the Leg.* Under Mr. BARNARD HOLT, at the Westminster Hospital. Reported by JOHN MILTON, Esq.—[The patient, a girl aged 24, suffered about fifteen or sixteen years ago, from a tumour about the size of a marble on the right foot under the inner ankle, which was removed by Mr. Liston, but the cicatrix remaining from the operation never became white and firm, but was always covered with a thin dry scab. About four years ago, a second tumour formed above the first, which gradually increased in size, and at last burst, discharging some matter. The part was poulticed, but no relief being obtained, Mr. Holt removed the tumour, which was about the size of a walnut.]

But it soon began to manifest greater activity, and to display its malignant and dangerous nature. The granulations, instead of being small and healthy, increased in size, and though attempts were made to destroy them by means of escharotics, they still grew larger, and a cherry-coloured nodulated mass arose as large as an egg: in washing it she constantly removed small, black coagula. Yet up to a late period she was able to walk; she could not bear a boot to be laced on the ankle, and setting the foot to the ground when she rose to begin walking was very painful, but this over, she could go pretty well. As there appeared no probability of saving the foot, she was made an in-patient on the 7th of May, and on the 23rd of June Mr. Holt removed the leg between the upper and middle thirds. On examination the disease was found to be confined almost entirely to the skin, having attacked none of the deeper-seated tissues, except the adductor pollicis.

[No untoward symptom occurred after the operation, except a gland which suppurated on the inner and upper part of the thigh.]—*Med. Times, Aug.* 10, *p.* 144.

96.—*Chimney-Sweeper's Cancer.* Under Mr. Holt at the Westminster Hospital. Reported by John L. Milton, Esq.—[It is stated that this disease is seen perhaps to a greater extent in the hospitals of London than in any other institutions in the world. Although it is supposed to proceed from soot irritating the scrotum, yet there are reasons to doubt whether the presence of soot or any other dirt is essential to the disease, as it is stated to have been seen in persons unexposed to the influence of soot; and the disease is unknown in Paris, where the lower orders of the population are infinitely filthier in their persons than the same class in London. In the following case, however, the patient had certainly been exposed to the action of soot.]

The disease first appeared like a small pimple, the top of which he repeatedly scratched off; by-and-by a little fluid oozed from the abraded surfaces, which gradually increased in extent till about four months ago. At this time its development seems to have received a sudden impulse, and it speedily increased in size. He began to dress it with poultices, which produced free suppuration, and gave him great relief. He never applied to any one for advice, so that the progress of the disease was quite uninfluenced by any foreign applications. He was admitted into the hospital, under Mr. Holt, on the 2nd of July, the growth having, by this time, attained the size of the tips of three fingers, and become very painful; the pain was of a lancinating character, and accompanied by beatings which gave him the sensation of the ticking of a clock.

As his health had now suffered considerably, he was operated on four days after admission, the incisions being made transversely. Some hemorrhage took place on the 8th, which impeded the progress of union by the first intention, but, by the 20th, the cicatrix had formed firmly, except over a minute line; the case may, therefore, be considered as having terminated favourably, though only a long period of time can assure us that there is no danger of relapse. The patient, however, leaves the hospital in a most gratifying state of improvement.

Respecting the mode of growth by which the scrotum is replaced there seems some obscurity. Mr. Liston says "it is astonishing how completely and in how short a time these glands (the testicles) receive a new covering at the expense of the surrounding healthy skin." The fact is well known, but we have not been able to meet with any very satisfactory account of the manner in which it occurs. In cases like that we have just recorded, where a small portion of the scrotum is removed, it is very easy to imagine how this structure, loose and extensile, may be greatly stretched by the pull of the ligatures; but there are facts which tend to show that independent of this the scrotum would of itself cover the testicles. Mr. Lynn on one occasion removed the whole of the scrotum, and yet the testicles were soon covered. In his wards there is now a man whose testicles were denuded by sloughing of a large portion of the scrotum, the consequence of exposure to cold and want. A short time ago, when we first saw this man, the testicles were exposed; three weeks after the edges of the scrotum had approximated within a very short distance of each other, though no mechanical means had been used to bring them together, the part having been only dressed with poultices, and now the superior portion of the scrotum has advanced downwards so far over the testicles that the lesion is only seen by looking over the projection formed by the lower end of it. May it not, then, be rather some remnant of that organic attraction which unites the two portions of the embryo at the mesial line, that performs this useful service, and thus remedies the loss of large portions of this membrane by disease or operation?—*Medical Times, Aug.* 10, 1850, *p.* 144.

97.—*On the Treatment of the "Warty Ulcers of Marjolin," by means of the Chloride of Zinc.* By Dr. H. FEARNSIDE, Physician to the Preston Dispensary.—In the May number of the Dublin Quarterly Journal of Med. Science, at the close of a valuable paper on the "Warty Ulcer of Marjolin," by Dr. Robert Smith, there occurs the following passage:—"Were I to attempt the cure of the disease by caustic, I would feel inclined to place the most reliance upon the chloride of zinc, the efficacy of which in destroying cutaneous cancer has been highly extolled by Dr. Canquoin, of Paris, and likewise by Dr. Ure.

[Dr. Fearnside proceeds to describe the case which fell under his care. His patient was a large unwieldly man, having the aspect of a man accustomed to indulge in malt liquor. About nine years previously he had sustained a severe injury in the front of his right leg. Inflammation and a swelling ensued the size of a pullet's egg, which underwent little change for some years. But 10 or 12 months prior to his coming under Dr. Fearnside, pain was felt in the tumour, which acquired a dark red colour, the skin covering it being indurated and uneven, and the discharge thin, fetid, and often bloody. Dr. Fearnside continues:]

When I first saw him, the morbid growth presented the following characters:—From the centre of the front of the right leg, occupying a space of about three inches in diameter, there sprung a dark grey substance, which projected at least two-thirds of an inch above the surrounding skin. Its surface had a granular appearance—at first view not unlike the head of a cauliflower: on closer examination, it was seen that this was occasioned by the prominent extremities of coarse fibres which arose from the base of the ulcer, and were collected into masses, separated from each other by deep fissures. The margin of the sore was thickened, elevated, and possessed little or no connection with the fibrous structure above described: the surrounding integument had undergone considerable warty induration and discolouration. A thin ichorous discharge proceeded from the part; the pain experienced was not violent; there was no enlargement of the popliteal or inguinal glands He complained of being weakened by repeated loss of blood, but his health was otherwise good. He had been previously under the care of one or two medical men, and had been advised to submit to amputation of the leg, on the supposition that the disease was fungus hematodes.

He was seen with me by my friend Mr. Noble, who considered the case a favourable one for the employment of the chloride of zinc; and, accordingly, it was determined to attempt the cure of the disease on the plan recommended by Mr. Alexander Ure.—(*Med. Gaz*, *vol.* xvii, *p.* 391.)

On its first application, the remedy was mixed with flour. (Canquoin's formula.) But little pain was occasioned by it, and the resulting slough was not deep. When it was next employed, it was blended with pure sulphate of lime, as proposed by Mr. Ure. The whole of the morbid growth was covered to the depth of a third of an inch with a paste composed of one part of chloride of zinc and two parts of sulphate of lime. The application gave rise to severe pain, which was only partially under the control of opium. At the expiration of four days an extensive and deep slough was produced, which separated in the course of the ensuing week or ten days. The greater part of the sore was then covered with healthy-looking granulations; but to get rid of two or three small masses of whitish semi-cartilaginous substance, as well as to overcome the indurated condition of the margin of the ulcer, it was requisite again to have recourse to the caustic paste. The subsequent progress of the case was, upon the whole, highly satisfactory. No hemorrhage ensued after the first complete application of the chloride of zinc. In the course of a month small florid granulations had arisen to the level of the adjoining surface, and cicatrization commenced. The complete healing of the sore was delayed for some time by the patient's imprudence; for, considering himself well, he could not be induced to submit longer to regular treatment. After the lapse of three months, however, a small ulcer of the size of a shilling was all that remained of the apparently formidable disease; and he afterwards reported himself as cured. *Med. Gazette*, *Sept.* 6, 1850, *p.* 411.

98. -ON THE MEDICINAL ACTION OF ARSENIC IN CUTANEOUS AND OTHER DISEASES.

By Thomas Hunt, Esq.

[Mr. Hunt asks, "Has arsenic ever been administered alone, alternately with other alteratives, in these diseases, for the purpose of comparing the results?" He replies, "It has."]

Cases 1, 2, 3.—Three cases of chronic eczema are reported in the *Medical Gazette* in the year 1841, under the care of Mr. Phillips, in the female ward of the St. Marylebone Infirmary. The patients were of the ages, respectively, of 38, 52, and 61. The two former presented at one period, on the hand and arm, the impeteginoid form of the disease. They were all treated first by tepid bathing every other night, and a drachm of sulphate of magnesia and ten minims of dilute sulphuric acid were administered three times a day for a month, with a certain, though small amount of benefit. Secondly, five minims of the tincture of cantharides were added to each dose. The relief now afforded to the younger patient was rapid and decided. and in six weeks she was discharged cured. In the other two cases no decided advantage was gained, although the dose was increased to fifteen minims thrice a day. Thirdly, both of these patients took five minims of the liquor potassæ arsenitis thrice a day. This produced, on both cases a striking effect in ten days, and by the end of the fifth week the disease was gone; slight œdema of the lower limbs only remaining, which soon yielded to bandaging. There is no room for doubting the efficiency of arsenic in these cases, else I could adduce from my own note-book, fifty cases equally conclusive, in which a chronic disease of the skin has been proof against the cantharides, the mineral acids, alkalies, iodine, tar, and other remedies of acknowledged power, and yet has yielded to arsenic. As I have already published many of these cases in my treatise on diseases of the skin, the above may suffice as the result of the experiment of substituting arsenic for another medicine of acknowledged power.

But perhaps the experiment of administering the medicine for a time, and then withholding it, leaving the case to nature, and afterwards resuming the use of the medicine, would be, to some minds, more satisfactory than any other mode of testing its therapeutical powers. This experiment I have seen so frequently tried, and with such generally uniform results, that I consider the power of arsenic over cutaneous disease as well established as the action of any alterative remedy—perhaps better. The following cases are in point. In some of them the dose was reduced, and in others the medicine was wholly suspended for a time, then resumed.

Case 4.—*Prurigo Scroti.*—A gentleman had suffered for many months, and in an intolerable degree, from this distressing affliction; and nothing had given him any relief. The disease was sthenic and inflammatory in its character, as indicated by the pulse, although there was no local inflammation apparent. Accordingly, after bleeding and purging had reduced the pulse, arsenic was administered as follows:—

1836. April 16th.—Liq. arsenicalis, ♏v. ter in die.

22nd.—No amendment. Liq. arsen. ♏vij. ter in die .

29th.—No relief, except from ablutions. Liq. arsen. ♏viij. ter in die.

May 1st.—Pruritus nearly gone; conjunctiva slightly inflamed. Reduce the dose to *five minims.*

9th.—The pruritus has returned in an annoying degree. Liq. arsen. ♏vij. ter in die.

17th.—No pruritus. Conjunctiva inflamed. Papulæ no longer visible. He considers himself cured. Discontinue the arsenic.

24th.—No return of the pruritus.

This case evidently yielded to arsenic, as proved by its obstinacy under a small dose, its improvement under an *increased* dose, its relapse when the dose was *reduced,* and its ultimate recovery on the full dose. The case is not adduced to illustrate the advantage of increasing the dose, for, generally speaking, this is bad practice; but to prove beyond doubt or cavil, the palpable control exercised by the medicine over the disease, which, to the best of my knowledge, has never relapsed.

Case 5.—Psoriasis.—A young lady, who had been troubled with psoriasis for several years, reported that she had formerly taken arsenic for it, but "it did not agree with her." She afterwards tried cantharides, but without any permanent or very obvious benefit.

Nov. 21st, 1838.—Liq. arsenicalis, ♏iv. ter in die.

May 12th, 1839.—Reports that the arsenic was of great service to her, but since she discontinued it the disease has returned as bad as ever. Resume the arsenic in four minim doses.

The disease was soon checked, but she again neglected the medicine, which she afterwards took, "off and on," for more than ten years, the disease invariably yielding most readily to every course, the eruption generally disappearing entirely after a few weeks, and returning as regularly when the medicine had been laid aside for a month or two. At length she was induced to take it with sufficient steadiness to justify hopes of the final subjugation of the disease.

Case 6.—Lepra Vulgaris.—A married lady, about 25 years of age had been troubled with lepra from infancy, and had found no benefit from treatment.

June 26th, 1845.—Extensive circular patches of lepra vulgaris on the arms and legs, especially the latter, and smaller patches over nearly the whole body. Liq. arsenicalis ♏v. ter in die.

July 12th.—Patches fading; conjunctiva tender. Reduce the dose to 4 minims.

20th.—Disease nearly well. Persist.

30th.—Quite well; no scales to be found, only brown stains. Persist.

Aug 19th.—She has neglected her medicine for three weeks. The leprous patches are breaking out again "everywhere." Resume the arsenic.

Oct. 3rd. –She has taken the medicine till now, and the disease has again disappeared. Persist.

Nov. 23rd.—She has discontinued the arsenic, and the spots are returning.

She now took the medicine more steadily, and the disease again disappeared. In the following summer a small patch made its appearance which got well under a few doses of arsenic, and I have not heard of her since that time.

Case 7.—Pityriasis Capitis.—A gentleman, aged 58, had a scaly disease of the scalp ten years ago, which was cured by the late Mr. Scott of Bromley (probably by arsenic).

Nov. 25th, 1847.—The disease has returned six months, and the patient can get no relief from medicine. There is an eruption of fine scales over the scalp, and on the whiskered part of the face. The rest of the body is free from disease. Liq. arsenicalis ♏v. ter in die.

Dec. 9th.—Eruption fading. Persist.

30th.—Nearly well. Persist.

Jan. 20th, 1848.—Conjunctivitis and catarrh. Reduce the dose to 3 minims.

27th.—Slight return of cutaneous irritation and scaliness. Take 4 minims for a dose.

March 9th.—Skin quite well.

In the following spring the disease returned, and was cured by arsenic in a very short time.

These cases are only adduced as samples of the direct-experiment mode of settling medical disputes; but it is right to observe, that the experiments were not tried empirically. It was first ascertained that the patient was in a condition favourable for the advantageous exhibition of arsenic. What that condition is was discovered by another series of experiments which will be detailed in the next paper; and this will explain the reason why there is so much discrepancy of opinion in the profession, as regards the use of arsenic, both with respect to its efficacy and safety.

The facts already stated, demonstrate (if there be anything demonstrable in medical science,) that arsenic is not only a very important and useful agent in the treatment of chronic diseases of the skin, but that it has very frequently succeeded after other remedies have been fairly tried. The next paper will disclose other facts, showing how it may be administered effectually after having itself failed to do any good when exhibited indiscreetly on the one hand, or timidly on the other.

[Having endeavoured to demonstrate the efficiency of arsenical medicines in chronic diseases of the skin, Mr. Hunt proceeds to enquire into the conditions on which that efficiency depends. He considers first—]

Conditions unfavourable to the administration of arsenic—1. A *febrile* state of the system, especially the sthenic form of pyrexia, indicated by the usual signs. If the pulse be frequent, and not easily compressed, the skin hot and dry, and the secretions scanty, and especially if there be chills and heat succeeding each other, arsenic will be found useless, or worse than useless; but, when this state of the system has been rectified, it may do good, as exemplified in the following case:—

Case 7.—Lepra Vulgaris.—A gentleman of full habit of body had suffered for about five years from numerous and extensive patches of lepra, occupying various regions of the body, and attended with intolerable pruritus, which had, of late, materially disturbed his rest. Arsenic was tried repeatedly, but without the slightest benefit; the irritation seemed to increase under its use. It was, at length, observed, that the pulse was rapid and incompressible, the skin hot, the tongue foul, and the patient, on being questioned, complained of thirst and feverishness. He was now treated with salines and aperients, continuing the arsenic as before; but, at the end of a fortnight, there was no improvement, the pulse continuing full. A pint of blood was now taken from the arm, and in less than a week he was much better; the skin became cool, the pulse quiet, the tongue clean, and the leprous patches assumed a faded appearance. The arsenic was continued for another month, by which time all the scales had disappeared, and the skin had become smooth. The disease returned in the following spring, and again yielded to arsenic, but not until leeches, cathartics, and a low diet, had prepared the system for its use.

In this case, as in many others on record, a general principle is involved, which applies to the treatment of all diseases attendend with sthenic inflammation, viz., to reduce the vascular action to the normal standard before attempting to carry out any special indication.

2. A condition of the circulation the very reverse of that which has just been described, has been known to present a limit to the salutary effect of arsenic, though by no means an objection to its use, as illustrated in the following case:—

Case 8.—*Psoriasis Diffusa.*—The skin of a girl of ten years of age exhibited extensive and irregular patches of a scaly disease which had proved obstinate under treatment; but on the administration of arsenic the disease rapidly improved up to a certain point. It then got as rapidly worse, and the child was shown to me. I found the pulse extremely weak, the appetite defective, and the patient languid and debilitated. The dose of the arsenic was reduced, and a grain of quinine, with some compound tincture of bark, was ordered to be taken twice a day; and with the return of appetite and vigour, which ensued under this treatment, the skin got speedily well.

3. A syphilitic taint in the system will render the disease incurable by arsenic, although in some cases it is beneficial in a degree.

Case 9.—*Syphilitic Lepra.*—A middle-aged married woman had a scaly eruption on the legs and thighs, which occasionally degenerated into fissures and ulcerations. There was a copper-coloured hue about the scales, and a dusky leaden appearance of the skin which had healed. A surgeon had treated it with arsenic with temporary benefit, not suspecting the existence of syphilis. Close inquiries, however, elicited, that although the husband was healthy, a sore on the right labium of the patient (the cicatrix of which was strongly marked) had resulted from connexion with a young man before her marriage, and this had been succeeded by an ulcerated throat. An energetic but short course of mercury was now administered, and in less than a month the skin was entirely free from disease.

4. The existence of organic visceral disease, as a complication of eruptions; an attack of diarrhœa, bronchitis, cynanche, coryza, or severe irritation in any mucous membrane; and certain conditions of the nervous system, especially those connected with dyspepsia; all these, and nearly all other disorders of the general system, generally contra-indicate the use of arsenic; but if the health can be restored by other means arsenic may afterwards be administered with advantage.

Case 10.—*Psoriasis complicated with dyspepsia.*—T. A., aged 22, had been from the age of seven years the subject of psoriasis, which in the spring and autumn was always much aggravated, extending over the greater portion of the whole surface of the body. He was likewise habitually troubled with headache, flatulency, and indigestion. He had been bled many times without advantage, and had taken arsenic in large doses for six months together. It made him sick, and he was consequently obliged to abandon its use, although the skin was in some degree benefited by it. The compound decoction of aloes, together with the compound rhubarb pill, administered for a few days only, completely relieved his dyspeptic symptoms. Arsenic was then administered steadily for seven weeks, and at the end of that period the skin was perfectly well, a slight degree only of sickness having been complained of during the last week of the course; but this subsided on discontinuing the mineral.

That the conditions above described are decidedly unfavourable to the exhibition of arsenic might have been proved by a multitude of cases, but few only have been selected as satisfactory samples of the rest. To these proofs may be added the testimony of many of my respected correspondents, who severally use the following terms as descriptive of the conditions which have been found, in their experience, incompatible with the advantageous use of arsenic, viz.:—Unsound lungs, natural irritability of mucous membranes, plethoric and inflammatory states, tonic condition of the system, sanguineous temperament, inflammatory tendency, tendency to sudden nervous exhaustion from any cause, irritability of constitution, acute diseases, proneness to irritability of the circulative or assimilative system, eczema mercuriale, head symptoms, organic disease.

Conversely, the conditions which have determined the respondents severally on the use of arsenic, or in other words, the indications for its exhibition, are comprised in the following terms, viz.:—The phlegmatic rather than the sanguine temperament; cachectic lax-fibred habits; when other medicines fail; in periodic diseases; debility; atonic state of the system; good general health; obstinacy of the disease; the presence of lepra; the chronic character of the disease; general pallor; slow circulation; general coldness; non-inflammatory condition; freedom from febrile or mucous irritation.

Mode of administration, dose, &c.—I. As arsenic is liable to irritate the stomach and bowels, and as this circumstance has been frequently considered a formidable objection to its use, it is important that it should be administered not on an empty stomach but rather with or after a meal. And there is another reason why this mode should be preferred. Dr. Golding Bird justly remarks, that as the elements of the food are absorbed by the lacteals and not by the veins; and as, on the other hand, the substances not forming part of the food are, when in solution, absorbed by the venous capillaries, arsenic or iron in solution, when taken on a meal, so as to mix with the food, will be almost entirely absorbed into the chyle by the lacteals; but if taken on an empty stomach the medicine will be chiefly absorbed by the venous capillaries, and enter the *portal* circulation. Now, as we wish the arsenic to enter the *general* circulation, it should be mixed with the food, or at least swallowed after a meal. The food present in the stomach not only dilutes the medicine, and thus preserves the mucous membrane from immediate contact with it, but in some measure provides for its direct transmission into the general circulation.

These propositions are so nearly self-evident, or at least so necessarily deducible from established physiological truths, as to require no cases for their demonstration.

II. Arsenic is a cumulative medicine, and therefore should never be administered in increasing doses; otherwise a sudden and sometimes an alarming development of its toxical effects will necessitate an entire suspension of the medicine. This is a most important point. Arsenic has been usually administered in increasing doses. It is directed in every work on the materia medica, and in every treatise on cutaneous diseases which have met the writer's eye, to be given in small doses *gradually increased* until some unpleasant effects become manifest, then to be *suspended for a time.*

A large majority of my seventy-five correspondents had been accustomed to administer arsenic in this way, and they record one or more of the following effects as the result of this method, viz.:—"Conjunctivitis," "swelling of the limbs or features," irritation of mucous membrane," purging," "gastritis," "desquamation of cuticle," "general inflammatory state," "flushings," "headache," "exhaustion," "restless nights," "sinking," "giddiness," "palpitations," "mental agitation and alarm." On the other hand I have been accustomed, for many years, to administer arsenic differently. Beginning with five minims of Fowler's solution, three times a day, and continuing that dose steadily until the conjunctiva or tarsi became slightly affected; then reducing the dose again and again as the cumulative action became apparent in the state of the *tarsi,* I have very rarely observed any of the more formidable results in the above catalogue of symptoms. And it is remarkable, that every writer who recommends increasing the dose, also very properly advises an entire suspension of the medicine when toxical symptoms appear. The consequence of this suspension is, that the disease relapses, and proves "obstinate and intractable." Whereas, if the dose be moderate and uniform in the first instance, and reduced when the conjunctiva becomes slightly inflamed, there will be no necessity for suspending the medicine at all; and by steady perseverance, we may rely upon the arsenic wearing out the disease without injuring the health; and, in fact, the health is generally found to improve under its influence.

The theory on which this practice is founded is very simple. If all subjects were equally susceptible of the action of the medicine it would be not only safe but advantageous to begin with at least twenty or thirty drops of Fowler's solution for a dose, this being the average dose borne without injury. But as we do not know what a patient will bear, different individuals varying considerably in their tolerance of arsenic, we begin with a moderate dose, say five minims three times a day. Now, a person taking this dose three times a day, will have taken in ten days, just 150 minims, and the effect of this on his system will be the same as that of taking 150 minims (minus the quantity excreted in ten days) at a dose. That this hypothesis is very near the truth I have demonstrated hundreds of cases.

Case 11.—Mr. Girdwood relates that a patient took two drachms of Fowler's solution in twenty-four hours by mistake. It cured the ague for which it was administered (a tertian), and it had no detrimental effect.

Case 12.—A lady, by mistake, took forty minims of Fowler's solution three times a day for several days: she felt no inconvenience for the first two days, but on the third and subsequent days her nerves became irritable, and in less than a week she was visited with trembling of the limbs, dimness of sight, sensations of exhaustion, and other affections of the nervous system.

Case 13.—A lady of delicate habit took, for the cure of a cutaneous eruption, five minims of Fowler's solution three times a day for three days; in all, forty-five minims. She then complained of a general tremor of the limbs, and inflammation of the tarsi.

Case 14.—One of my correspondents administered ten minims of the solution three times a day for two or three months without the slightest sensible effect being produced, either local or general.

These four cases show that the susceptibility of arsenic varies much in different subjects; that where the susceptibility is not great a dose of two drachms of the solution can be borne about as well in one dose as in twenty; and that where the susceptibility *is* very great, forty or fifty minims given in divided doses for three or four consecutive days will produce an effect on the system, clearly cumulative. It is not necessary to say another word in favour of this mode of administration, since it is evident that both the safety and success of the medicine will mainly depend upon it.—*Med. Times, Aug.* 10, *p.* 140, *and Sept.* 14, 1850, *p.* 279.

99.—*Treatment of Lupus.* By Dr. THOMSON. From a practical treatise 'On Diseases affecting the Skin.'—In the treatment of lupus, Dr. Thomson was remarkably successful; and we believe that we are only doing an act of justice to his memory, in giving the following sketch of the plan adopted by him in the management of this very formidable and intractable disease.

He used but few local applications, and relied chiefly on three medicines, viz , iron, iodine, and arsenic, together with biniodide of mercury, and the occasional use of cod-liver oil and quinine. When a patient first came under treatment, if the general health were at all lowered, and if there were any anemic symptoms, cod-liver oil and iodide of iron were given for ten or fourteen days, the diet being at the same time nutritious, and other means being adopted to invigorate the system. Afterwards, the alternative treatment commenced with the administration of the biniodide of arsenic in doses of one-twelfth of a grain; and Dr. Thomson considered it the safest plan to adhere to these small doses, as in large ones it often produces gastrodynia, and when this has once occurred, the stomach is extremely intolerant afterwards of the smallest dose. If any gastrodynia came on, the medicine was at once left off, and opium and hydrocyanic acid were administered. Conium was after found useful, and was frequently combined with the biniodide. If the latter, however, could not be borne in any form, the liquor potassæ arsenitis, of the London pharmacopœia, was given in small doses of from four to eight minims, and iodine was rubbed in over the healthy skin to promote absorption. Although local caustic applications were very little employed by Dr. Thomson, yet he occasionally applied the strong nitric acid to the edges of the ulcer, or a strong solution of nitrate of silver (ʒ ij or ʒ iij to ℥j of water) over the tubercles, and subsequently acetate of lead-wash to lessen the temporary heat and swelling which followed. If unhealthy, pale, fungous granulations arose, an ointment of iodide of sulphur, or a very weak ointment of the biniodide of arsenic, or of either of the iodides of mercury, was employed. Four cases are recorded in the work, in which the above treatment was adopted, and with perfect success in every instance.—*London Journal of Med. Science, September,* 1850, *p.* 847.

100.—*Ulcer of the Leg, treated by the Internal Use of Turpentine.* Reported by W. Hamilton, Esq., House Surgeon to Charing-Cross Hospital.—[The following case illustrates the observations made by Mr. Hancock on the great utility of turpentine in some forms of ulcer of the legs, especially depending on debility, with want of action in the system, circulation languid, wound sluggish, with smooth surface, and of a greenish foul appearance, without granulations. E. B., a laundress, who had never menstruated but at the monthly periods, had coughed up and vomited blood, was admitted with an ulcer on the leg, nearly five inches broad, extending almost completely round the limb. It was in the condition before mentioned, and accompanied by excessive pain. Her health was extremely delicate—she had wasted away—lost appetite—was subjected to night sweats, with a terrible cough.]

Under these circumstances, Mr. Hancock, under whose care she was placed, commenced giving her the cod-liver oil; but it caused so much nausea it had to be discontinued; and on the second day the turpentine mixture was ordered as follows:—

℞. Sp. terebinth., ʒ vi.; pulv. acaciæ, ʒ vi.; aq. menth. pip. ft., mist. ℥ viii.; cap. ℥ i. ter die.

At the same time she had a generous diet, with a daily allowance of wine. Linseed-meal poultices were applied to the leg for the first day or two; but afterwards, throughout the case, water-dressing, and the leg bandaged up to the knee.

Under this treatment she continued to improve daily, the wound became covered with small regular granulations, secreting good healthy pus. Her appetite improved, the menses returned, and with it ceased the expectoration of blood. She was discharged, with the leg quite healed, June 15, just three months after her admission.—*Med. Times, July* 6, 1850, *p.* 12.

101.—*Treatment of Varicose Ulcer at St. Thomas's Hospital.* Reported by Dr. Charles Kidd.—[The healing of an ulcer of the lower extremities, though apparently simple, is one of those difficult problems which so often test the patience, as well as exercise the skill of the surgeon; and among these the varicose ulcer holds a prominent position. As an example of this, Dr. Kidd describes the following cases. The first was that of Henry Cooper, aged 36, who came into the hospital July 26, having been seven weeks ill with an old sore leg, all the side of the ankle being now one large ulcer with varicose veins.]

Having undergone much of the usual preliminary treatment before coming into hospital, in addition to purgative medicines having had ointments and lotions, &c., it was thought advisable to check if possible the varicose condition of the vessels. The old plan of taking up the vein and tying it, always leading to phlebetis, was thought not advisable. A caustic issue along the chief vein, between the ulcer and the heart, a plan tried with the best results previously in this hospital, was adopted, and has proved most satisfactory. It has quite astonished the poor man himself, who hopes to get back in a few days to his employment. The potassa fusa has of course produced a large eschar, to which he has applied a poultice. The ulcer is carefully strapped and bandaged, as well described by Mr. Chapman, and rest with the horizontal position enjoined.

August 4.—The man seems quite rejoiced at the sudden improvement in his health and the healing up of his old ulcer. The issue is not so troublesome to-day as it has been; poultice to be continued to it; plaster and strapping to be renewed.

6th.—Ulcer visibly healing; the issue a little sore. His general health and strength have undergone the most marked improvement. The most remarkable point, however, in the case is the quickness with which healthy granulations in the old ulcer have sprung up.

Case 2.—Jeremiah Crawley, aged 40, admitted July 10, is the counterpart of the foregoing case; he has been laid up with a bad leg for seven months, during which he has been able to work very little, but has at last decided on coming into hospital. He has large varicose veins, and the usual indolent ulceration near the ankle, on the inside of the leg, with large indurated callous edges. His general health seems excellent.

He was ordered the usual house medicine, to have the leg carefully strapped and bandaged, and the *potassa fusa* rubbed along over the site of the larger vein near the inside of the knee; to give the limb as much rest as possible. This man, too, has been going on most favourably; the ulcer is just healed upon, and the varicose condition of the veins is quite removed. He goes out in a few days.

The *modus operandi* of the caustic in these cases is, perhaps, open to a little speculation. Mr. Green seems to think it destroys the superficial varicose veins by interrupting the current of blood in them, and forcing it into the deeper veins. If we succeed in checking this varicose condition of the vessels under the skin, the healing of the ulcer follows as a matter of consequence; we have possibly the counter-irritant effect of the caustic also. The great practical fact, however, for our notice is, that it is the best mode yet tried, and the safest, perhaps, for healing those very troublesome and very unsatisfactory ulcers. It is less dangerous than any of the plans recommended for interfering directly with the vein, and I am not quite sure but it is simpler, and more grateful to the patient.

Taking up the saphena vein was one time tried in these cases, but is now very properly discontinued. The cutting out of a piece of the vessel seems not less formidable, and is equally dangerous. The subcutaneous incision of the vein, as practised by Sir B. Brodie, has been tried also; but his own very clear account of his cases would not lead us to put much faith in the operation. Then we have *puncture* of the vein, of which little can be said that is favourable,—*cutting* the vein, piercing it with needles, &c. The most simple practice, and certainly that liable to injure the constitution the least, is the use of the caustic potash. Of course, before having recourse even to this, it would be advisable to give a fair trial to the simple use of a well-applied bandage, with water dressings or the adhesive straps, according to the exact condition of the ulcer. If evidently inflamed, as the ulcers mostly are when first seen, the patient should be kept in bed, fomentations, and perhaps leeches, tried. When the inflammation is got under, the ulcer is to be covered with a little soft charpie; the varicose condition counteracted, as much as possible, by a skilfully applied bandage, affording equable support from the toes to the knee. Sticking-plaster, in single straps, (like an ordinary Scultetus's bandage,) or after the plan of Mr. Chapman, is, perhaps, preferable to the classical spirals of Baynton's method. Great care is necessary in the application of the bandages and straps, as well as that of the laced stocking; the latter should be such as not to retain heat too much,—some light fabric, calico, stocking-web, or nankeen being the best. The caustic potash, I need scarcely say, was originally suggested by Bonnet, and in this country first tried, perhaps, by Mayo. The chief objection against it is that, more than once urged, that the slough takes a long time to separate; and when one cluster of veins is lessened in size another cluster appears. The mere fact of its being almost exclusively used in this and other hospitals would lead us, however, to doubt if there is much practical force in the objection. The introduction of a thread through the vein to produce a clot has been suggested where the caustic has failed. A good plan, also, is to thrust a pin under the large vein and raise it, a second pin being thrust through it crosswise, a thread of silk twisted round both externally. This obviously will produce a clot, and should be had recourse to after the simpler means we have alluded to have failed. Some ordinary phlegmonous inflammation will follow, with swelling of the inguinal glands, not of very great moment if watched. In old people any operation of veins is dangerous. In young patients with bad varicose ulcers, after the caustic has failed, the pins are worthy of a trial, and should at least precede the more formidable and more dangerous operation—that of tying the vein. Every practical surgeon must at once recognise the difficulty of giving any general rules; some patients not bearing the slightest puncture or cut, and others as commonly escaping after the most formidable operations. In all we have, however, a safe resource in the potassa fusa, as we have just described.—*Med. Times, Sept.* 7, 1850, *p.* 255.

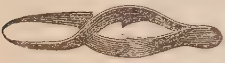
102.—*Removal of Deformity after the Cicatrization of a Wound caused by a Burn.* By W. FERGUSSON, Esq. —[The removal of this deformity by manual operation is well evidenced in the following interesting case, under Mr. Fergusson.]

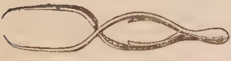
The patient, who was a little girl about ten years of age, had been burned some time ago in the left arm. The injury affected the parts just in the flexure of the elbow-joint. It is probable that so much precaution as was necessary in these cases was not taken; for as cicatrization had ensued, the elbow became contracted by the firm puckering of the integuments. In order to set this free, Mr. Fergusson performed the following operation:—By two nearly parallel incisions he included the old cicatrix, which was about three inches in length, and dissected it cleanly away from the tissues beneath. Another incision, at right angles to the former, about an inch in length, was then made, for the purpose of taking off the tension of the parts, and the edges of the wound were brought lightly together with thin sutures. This operation, although simple in appearance, requires great neatness and care in its execution; and much precaution is necessary in the after-treatment of such cases, as there is always a very great disposition in the parts to contract after they have been liberated. Mr. Fergusson stated that the plan he had just adopted was very similar to the one recommended by Sir James Earle, and that it was the most likely to be successful in the removal of such contractions.—*London Med. Gazette, Aug.* 2, 1850, p. 211.

103.—*On the Effects of a Bread and Milk Diet on a supposed Malignant Tumour of Eight Years standing.* By Dr. Bowditch.—This case occurred in the person of Dr. Twitchell, one of the most celebrated medical practitioners in New England. Carcinoma had prevailed in his family, and during his studentship he had suffered first from dyspepsia, and then from severe asthma, which entirely left him after he had abandoned the use of meat, and confined himself to a vegetable diet on account of an acne with which he was troubled. After nine years of such abstinence, he gradually resumed the moderate use of meat. The local disease in question commenced about eight or ten years since, as a small hard tumour at the inner angle of the eye, which slowly but gradually enlarged so as to become very conspicuous. The greater part was excised, but the wound did not heal, and another operation, followed by the use of nitrate of silver, was performed. Eventually the ulcer which resulted assumed a decidedly malignant aspect, and the general opinion of the eminent men he consulted at the meeting of the Medical Association at Philadelphia, in May, 1847, was that the disease was of a very serious character. A variety of local applications having been tried, Dr. Twitchell, starting from the theory that malignant disease arose from the taking too much carbon into the system, resolved to limit himself to a bread and milk diet, and to this he henceforth strictly adhered. He took from four to six ounces of cream, or the richest milk, and as much white or brown bread, three times daily. The immediate results were a cessation of the pain, a diminution of the discharge, and an arrest of the spread of the ulcer—this eventually diminishing, and then altogether disappearing. When Dr. Bowditch saw him, August, 1849, no difference could be discerned in the angles of the two eyes, unless by a person already aware of the former existence of the ulcer, who would then perceive a small soft cicatrix. A perfect cure of what all thought to be a malignant disease of ten years' duration had thus been accomplished by this diet, commenced when the patient was 68 years of age. Under its influence too, Dr. Twitchell found himself less irritable than heretofore, while, on account of a tendency to corpulence under its use, he was at one time obliged to reduce the quantity. His strength continued as great, though having a large practice to attend to; his digestion good and his respiration more free. To the eye he had the appearance of a hale, robust man, in perfect health.—*Am. Journ. Med. Sc.*

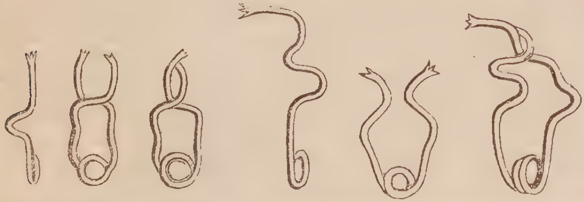
By an obituary notice in the same journal, we perceive that Dr. Twitchell died in May, 1850; but, as that notice is contributed by another hand, there is no account of his health since Br. Bowditch's communication in November, 1849, nor is the cause of his death stated. —*Brit. and For. Med.-Chirurg. Review, October,* 1850, *p.* 553.

104.—*Union of Wounds.* By M. VIDAL.—[At various periods in the history of surgery other means besides the employment of adhesive plasters and sutures have been employed to promote the union of wounds, and M. Vidal finding that the wounds from the operation of phymosis completely healed in twenty-four hours, he had made for him a small pair of forceps to keep the edges of the wound in contact, without its actually perforating the integuments.]





In this little forceps, from the manner in which the blades cross each other, the points are constantly kept in apposition, until a slight pressure is made at the part where both are united. M. Vidal has proposed the name of "serres-fines" for these little instruments, which in his opinion are destined completely to supplant the ligature and the use of adhesive plaster. The number of them to be employed in any particular case will depend on the extent of the wound. "It may be necessary," says M. Vidal, "to apply as many as sixteen." The period for which this application is to be continued, does not, in the opinion of this surgeon, exceed twenty-four hours; and he states that the re-union is accomplished as completely after eight as after twenty-four hours. They can be removed with facility, it being always remembered that they open or close by a mechanism exactly the opposite to that of the ordinary forceps. In their removal they present many advantages over the suture, occasioning neither pain, bleeding, nor traction. Their employment is not limited to wounds of the prepuce. The inventor has himself used them for the union of a wound of the temporal region. M. Danyau gives a successful case of their employment in rupture of the perineum, of twelve hours' standing, in which union was accomplished in sixty-five hours. Their application has been proposed by Vidal, in the case of varices, with the view of arresting the current of blood, without injury



to the integument or the vein itself. In a subsequent communication to the 'Bulletin de Thérapeutique,' the author has proposed a modification of his *serres-fines*, by which greater simplicity, and, what is more important, lightness, are combined. They are formed of silver wire, of the strength of an ordinary pin, and are easily made: the above figures exhibit several varieties of them.—*Dub. Quarterly Journal of Med. Science, Aug.* 1850, *p.* 119.

105.—*Collodion in Erysipelas.* Under the care of Mr. Luke, at the London hospital.—We have to direct the attention of our readers to a new topical application in cases of erysipelas, the beneficial effects of which are now placed beyond a doubt. The substance in question is collodion, which has been found so serviceable in many respects, not the least being its capability of arresting the hemorrhage from leech-bites in children. Mr. Luke considers that collodion applied to the inflamed surface in erysipelas, acts by compressing the capillaries of the skin, and thereby contributes materially in relieving those vessels.

He was induced to make use of this liquid in the manner described, on the suggestion of Mr. Bird, the author of the Jacksonian Essay on Erysipelas, which lately gained the prize: and Mr. Luke has had occasion to be highly satisfied with the results he has obtained, both in hospital and private practice. Several cases of erysipelas have been thus treated in Mr. Luke's wards, it will, however, be sufficient to report one of these, kindly furnished by Mr. Peete, to show how fully the application answers the purpose.

Amelia S., forty years of age, was admitted under the care of Mr. Luke, June 1, 1850, with acute inflammation of the neck, which, a week after admission, ended in abscess. This was opened, a large quantity of pus evacuated, and the patient went on very favourably for a week, when an inflammation of an erysipelatous character was observed to have invaded the upper part of the back. It extended from the neck to the first lumbar vertebra, including both scapulæ, and the inflammation had a distinctly defined margin.

Collodion was immediately applied by means of the finger to the whole surface, and to some little distance beyond it. The skin was much puckered by this measure, and the patient complained of the constringent effects of the collodion. Mr. Luke ordered the patient to have an allowance of wine, as she was rather depressed. (We may here state that most cases of erysipelas are treated in this hospital by large doses of stimulants, as brandy, wine, &c. &c.; and that this line of practice is generally followed by very favourable results. The type of the erysipelatous affection in charitable institutions is commonly of a very debilitated kind, and this fact would partially explain the necessity of supporting the patients by the stimulants just mentioned.)

In the case before us, the collodion was repeated on the following day, and on the third, the erysipelas had entirely subsided in the part where it had first made its appearance. It, however, broke out afresh about the nose, lips, and eyelids; and a little lower down it extended from the edge of the wound made by the lancet to the whole chest, implicating both mammæ. Patient complained of violent pain in her head; her eyelids were closed; and she had delirium at intervals, particularly at night. The collodion was applied, in the manner described above, to the whole chest and face; the hair was removed, and the head kept cool with a spirit lotion. These topical measures, combined with the administration of stimulants, proved extremely beneficial, and the patient improved rapidly. The collodion was applied daily for a week, and on the 24th of June, ten days after the first onset of the erysipelas, the inflammation had quite disappeared, and the patient was declared convalescent.

A male patient was lately treated in the same manner for erysipelas, and the successful results were very quickly obtained Mr. Luke has used the collodion very frequently in private practice: in one instance it was placed upon a young lady's face, excepting a small portion of the cheek, from which it peeled off. This part, soon afterwards, begun to look redder, and projected beyond the surface of the surrounding skin, being, in some degree, herniated. This circumstance showed very clearly how great must have been the pressure which was exercised by the fluid. Thus it would appear that the collodion fulfils two indications of an important kind: it protects the inflamed surface from the contact of the air, and it contributes, by the pressure it effects, in driving the blood from the distended capillaries. —*Lancet, July* 13, 1850, *p.* 60.

106.—*Medicated Soaps for the Treatment of Skin Diseases.* By Sir H. Marsh.—Sir H. Marsh recommends the following in the more chronic forms of skin disease, as chronic eczema, psoriasis, &c.: —

Sulphur Soap.—Take of white Windsor soap two ounces, spirit of wine, coloured with alkanet root, one fluid drachm. Pound the soap well in a marble mortar, so as effectually to get rid of all lumps; add the spirit, and beat into a uniform paste; then add sublimed sulphur two drachms, otto of rose ten drops—beat all well together.

White Precipitate Soap is made precisely as the sulphur soap, substituting ammoniated submuriate of mercury for the sulphur.

Red Precipitate Soap.—Take of white Windsor soap two ounces, rectified spirit of wine one fluid drachm; pound the soap well in a marble mortar until all lumps have disappeared; add the spirit, and beat into a uniform paste; then add precipitated red oxide of mercury one drachm, otto of rose six drops; beat all well together.

Corrosive Sublimate Soap.—Take of white Windsor soap two ounces, spirit of wine one fluid drachm, corrosive sublimate one scruple; pound the soap well in a marble mortar until all lumps have disappeared; add the corrosive sublimate, previously reduced to a fine powder and rubbed in a separate mortar, with the spirit. Beat all into a uniform paste, adding six drops of otto of rose.

Sir H. Marsh states that the only forms of cutaneous disease that admit of the application of these soaps are those which either have passed from the acute into the chronic stage, or have not been at any period peculiarly irritable and tender; at the same time, he has met with several cases in which the tender surfaces, at first intolerant, became subsequently inured to them; and, after a little perseverance, the patients began to speak highly of their soothing and beneficial effects.—*Dublin Med. Press, Sept.* 4, 1850.—*Prov. Med. and Surg. Journal, Oct.* 16, 1850, *p.* 581.

SYPHILITIC DISEASES.

107.—ON THE TREATMENT OF SECONDARY CONSTITUTIONAL SYPHILIS.

By Langston Parker, Esq., Birmingham.

[The intention of Mr. Parker's work is to bring under the notice of the profession the method which he employs in the cure of constitutional venereal diseases—requiring a small amount of internal medicine, conducted without risk, and in a vast majority of instances with certainty of success, and permanent cure in a short time. It is denominated the mercurial vapour bath, "consisting in surrounding the patient with an atmosphere of mercurial vapour in a moist state," and having, according to Mr. Parker, all the advantages, and none of the disadvantages, of the best conducted ordinary modes of mercurial treatment. The mode of applying it is as follows:—]

The patient is placed on a chair, and covered with an oil-cloth lined with flannel, which is supported by a proper frame work. Under the chair are placed a copper bath, containing water, and a metal plate, on which is placed from one to three drachms of the bisulphuret of mercury, or the same quantity of the grey oxide, or the binoxide. Under each of these is a spirit lamp. The patient is thus exposed to the influence of three agents,—heated air, common steam, and the vapour of mercury, which is thus applied to the whole surface of the body in a moist state. After the patient has remained in the bath from five to ten minutes, perspiration generally commences; and, by the end of twenty or thirty minutes, beyond which I do not prolong the bath, it is generally excessive. The lamps are now removed, and the temperature gradually allowed to sink; when the patient has become moderately cool the coverings are removed, and the body rubbed dry. The patient is suffered to repose in an arm-chair for a short time, during which he drinks a cup of warm decoction of guaiacum, sweetened with syrup of sarsaparilla.

[Very little medicine is required to be administered internally during the use of the baths, and Mr. Parker states, "I never saw the health of the most delicate patient, either male or female, injured under the plan I recommend," and "I have rarely seen a disease that has not been cured without a relapse." This plan of treatment being introduced by a surgeon of Mr. Parker's standing, renders it imperatively the duty of the profession to give it a fair and extended trial]—*Med. Gazette, June* 21, 1850, *p.* 1082.

208.—ON THE PRESENT CONDITION AND TREATMENT OF VENEREAL DISEASE IN PARIS.

By WILLIAM ACTON, Esq.

[Mr. Acton commences by stating, that although the medical journals every now and then describe any new plan of treatment M. Ricord may adopt in venereal diseases, yet that they give but an imperfect idea of the progress which medical science is making. He says:]

The first thing which strikes the most casual observer, on looking over the cases, is the large proportions of indurated chancres—thirty-three cases in the wards at one time. Here the student may study induration in all its forms. The frequency of this symptom in the wards depends upon M. Ricord admitting by preference, as patients, those who suffer under this form of chancre. Like all writers on syphilis, he lays the greatest stress on this symptom: but differing from all his predecessors, he does not consider all hardness attending a chancre to be induration. To diagnose this true symptom of syphilis, he looks for, and points out to the pupils, the enlargement of the glands in the groins, occurring a few days after the induration on the penis. These buboes rarely suppurate; when matter forms it is the result of inflammation superadded to the disease, and is an accident, whereas the enlargement in the groin is a constant attendant upon indurated chancre. When this symptom is not to be detected, he often keeps the patient in the wards, to show that the false ulceration will subside of itself, and secondary symptoms not occur.

I said above that you may study, in these thirty-three cases of indurated chancre, the disease in all its varied consequences. In by far the majority of cases, secondary symptoms had already appeared; and on reference to the table, twenty-one cases are noted down in which secondary symptoms were the most prominent feature; here indurated chancre had previously existed. In these cases of constitutional syphilis, M. Ricord dwells particularly on a character not noticed by previous writers—namely, enlargement of the glands at the back of the neck, a very constant occurrence in secondary symptoms, and one that comes on early—say five weeks—after general contamination of the system. The treatment of all these cases consists in ordering the mineral, in the form of proto-ioduret of mercury, a preparation he almost exclusively gives, but one which in England I have been unable to prescribe, as the same pills which patients bring from Paris act too violently on the bowels in this country, inducing colic and diarrhœa,—consequences rarely seen in Paris, and which we must attribute to climate and difference in diet in the two capitals. I need not here dwell on the long period mercury, he thinks, must be given in cases of indurated chancre or secondary symptoms, and be continued even after their disappearance; three or six months' persistence will scarcely be long enough to eradicate the syphilitic diathesis, and prevent relapses when the constitution has been once infected. All his further experience has only convinced the professor of the 'Hôpital du Midi' of the correctness of his ideas long since given to the public, but he now further assists their action by prescribing more frequently.

Fumigations.—This mode of giving mercury is so rarely followed in London, at the present day, that perhaps few of my readers are aware of the means employed.

The patient is placed naked in a box, his head being the only part exposed to the air. This box is heated by a furnace below, in which the bisulphuret of mercury is placed, in proportions of three drachms. The heat soon volatilizes the mineral, and the fumes come in contact with the skin. The patient is allowed to remain twenty minutes in the box, and a tumblerful of water is given him. In a few minutes perspiration comes on, and the patient is then taken out, and well rubbed down; and the fumigation may be repeated twice a week for a month or six weeks, according to circumstances. In old chronic affections which do not yield to mercury, given internally, the most marvellous effects are often produced, and I have seen cases (resisting all other treatment) get rapidly well under this plan; and I would recommend a trial to my professional brethren in the numerous instances that come before them of obstinate complaints. I am, however, by no means convinced of its general application, or that it is likely to supersede the mineral, given internally. It has been said that patients may employ fumigations, and yet pursue their ordinary occupations. One case that has lately come under my notice induces me to recommend that patients, after employing the cinnabar, should not expose themselves to the open air. I fear, likewise, that in country practice the use of it must be circumscribed. A fixed apparatus will be necessary, as the volatilization of the mercury is difficult, in consequence of the great quantity of heat required. The ordinary spirit lamp appears insufficient. I lately placed a patient in a vapour-bath, and after weighing three drachms of the cinnabar, placed it on the plate, over a strong spirit-lamp. In twenty minutes the substance had only lost one scruple in weight, independently of waste, thus showing that this is a very imperfect way of volatilizing the mercury, the vapour of which is so heavy that it will not rise more than one foot, unless carried up by a strong current of air. These peculiarities explain why fumigations have fallen into disuse, as the proper means of application are not always at hand.

Tertiary Symptoms may be seen in the wards in all their variety; and I noticed one which will form the subject of a separate communication—viz., of amaurosis, depending upon disease of the sphenoid bone. Suffice it, at present, to say that iodide of potash is our sheet-anchor, given in *large doses*. It is a very general belief in London, among our leading surgeons, that all the good that iodide of potash can do, will accrue if three or five grains be given, three times a day; more than that I am daily told, will do harm, or if more be given with impunity, the article is spurious. I do not wish to underrate the good effects of the salt often resulting from small doses, but I am as equally convinced that large doses may be given with impunity. I lately had a gentleman from Scotland under my care, who took the remedy in such quantities that he purchased it by the half-pound, and yet it was a genuine article; but I will go further than this, and assert, from a pretty large experience of its effects, that small doses do not do any or the slightest good in many instances. I have now under my care a patient with affection of the nose, and destruction of the ossa nasi, who had been taking small doses of iodide of potash for many months, under a late lamented surgeon, and the disease remained pretty nearly in *statu quo*. She came under my care, and the affection is now on the point of complete recovery under increased doses.

I met Mr. Wallace in consultation a short time since, in reference to a case of tertiary symptoms of the nose and brow, which iodine had relieved, but not cured, although taken in these small doses by the direction of another surgeon for nine months; here the salt had been obliged to be left off, because iodic intoxication was said to have been produced, together with symptoms of affection of the brain. Now, in these instances, time, rather than the dose, was in fault, and the surgeon who entertains these ideas scruples not to prescribe iodide of potash for years, rather than give it in larger doses.

I saw, with Mr. Vickers, a gentleman who has now entirely recovered, to whom we gave large doses of iodide of potash with bitters, yet small doses had failed in curing him, although given for long periods under the advice of other practitioners. I mention these cases because prejudices exist against the employment of the remedy, whereas, in France, Ricord gives it in anything but homœopathic doses, and with the most signal success.

Phagedenic Chancre.—At the time I visited the wards, only two cases existed of this disease, but generally it is much more frequently met with. Under this head may be included all those anomalous sores met with in London, or elsewhere, which are classed as irritable chancres, occurring in bad constitutions, indisposed to heal, spreading, instead of healing kindly, and causing the greatest annoyance to both surgeon and patient. In consultation practice in London, these are the cases I am most frequently consulted about to sanction the use of mercury. All sorts of washes have been used, caustic has been applied, but without avail, and the case is getting worse. In all such instances iron is the remedy, and its effects are perfectly magical. I need not say that mercury aggravates such cases. Of all preparations, the tartrate of iron is best, given in the following manner:—potassio tartrate of iron, one ounce; water, six ounces. Mix. Two tablespoonfuls three times a day.

M. Ricord largely employs it, and prescribes it now, even in the treatment of simple cases of chancre: he uses it, likewise, as a local application, as well as an internal remedy.

Chronic Catarrh of the Bladder.—Ricord now uses caustic injections into the viscus itself. Having emptied the organ of urine, he passes a gum-elastic catheter, and then, with a glass syringe, throws the injection into the bladder, and repeats it, according to circumstances, every three or four or six days. There is some pain and irritation set up; the urine becomes a little bloody, but the ropy secretion soon ceases. The different preparations of turpentine are given, and I saw four cases in their different stages. He has not met with any ill consequences of the treatment, although the injections used are composed of nitrate of silver, two drachms; distilled water, four ounces. Mix for an injection.

Strictures.—On the day I took notes of the cases, only three instances of stricture of the urethra presented themselves in M. Ricord's wards, and one of urinary fistulæ. The whole of these were serious examples of their respective diseases, and formed the subject of a clinical lecture. I am happy to say, that the eminent professor of the Hôpital du Midi corroborated many of the opinions I expressed during the last winter, on the subject of treatment. In the ordinary cases, he employs simple dilatation by means of conical gum-elastic bougies, with a little olive-shaped point. When retraction takes place, the case is treated by incision with instruments passed down the urethra, and he has lately introduced several modifications in the shape of the instruments with which the stricture is to be incised. Nitrate of silver is rarely employed in stricture; and on the subject of potassa fusa, M. Ricord agrees with modern surgeons that in small quantities the kali becomes saponified, and is insufficient; that in larger masses, when pressed against the obstruction, it is attended with the most serious consequences, and is not a remedy at all adapted to overcome the impassable stricture which every now and then comes before the surgeon.

I mentioned to M. Ricord the fatal results which had in London attended the operation of incising the perineum as recommended by Professor Syme. He thought that one or two unsuccessful cases should not deter us from prosecuting the plan, provided all others had failed.—*Lancet, July* 13, 1850, *p.* 51.

109.—*Cases of Gonorrhœa treated by Acetate of Potash.* By JOHN HILTON, Esq. F.R.S., Guy's Hospital.—[Copaiba and cubebs so commonly used in the treatment of gonorrhœa, however the form of their administration may be varied, are of so disagreeable a nature, that praise is due to those who seek to produce their beneficial effects by remedial agents of a more pleasant and palatable kind. The following cases of the treatment of this disease by the acetate of potash alone will not therefore, it is hoped, be uninteresting.]

The first case refers to a man, aged thirty, a tailor by trade, who, when admitted, July 24th, into Samaritan ward, had been affected with a discharge for a week; the urine was very acid, and the scalding intense The day after admission, Mr. Hilton prescribed half-drachm doses of acetate of potash to be taken three times a day. Five days afterwards, the scalding was relieved, the urine being still acid; on the thirteenth day, the scalding, though decreased in intensity, continued, and the urine remained acid; the acetate was therefore ordered to be taken every fourth hour. On the sixteenth day, the scalding was trifling, and the urine acid; on the eighteenth, the urine became alkaline; the scalding had disappeared, and the discharge had completely ceased for some days. The patients are always kept in the house a little time after its cessation, for fear of a relapse; and on the twenty-third day the patient was quite well, after having taken the acetate of potash three times a day for thirteen days, and every four hours for five days.

The second case is that of a bricklayer, aged twenty-five years; his affection may be looked upon as of a gleety character, for when he was admitted the discharge had been of nine weeks' duration. The half-drachm doses of acetate of potash were began at the same time as in the preceding case; on the third day the scalding was better, and the urine acid; on the fifth the scalding was gone, and the urine neutral; on the sixth, the urine was alkaline; and on the eighth the discharge had completely ceased. This patient was therefore relieved of all his symptoms after having taken the acetate of potash three times daily for nine days, the result offering a striking contrast with the first case. As to the diet, Mr. Hilton considers that it ought to be unstimulating, and that the use of strong beverages is to be especially avoided.

The third case is that of a labourer, twenty-seven years of age; he had had a gonorrhœal discharge for three weeks when he applied for admission. The treatment began in the same ward, (Samaritan,) on the 24th of July, as in both the preceding cases, and the acetate of potash, in an aqueous menstruum, was likewise ordered to be taken three times a day. The salt is also given with advantage in camphor mixture, and Mr. Hilton often supplies the patients with the acetate in powder, desiring them to dissolve it in water themselves. Here, however, no very material improvement took place for the first fifteen days, when the scalding became less, and the discharge began to diminish in quantity, the urine being acid. Mr. Hilton now ordered the same doses to be taken every fourth hour. On the nineteenth day the scalding continued, the discharge was less, and the urine slightly alkaline. On the twenty-second, the urine became again acid, though the patient was taking the acetate every fourth hour. At this time diarrhœa set in, and the patient was precluded from continuing the acetate of potash. On the twenty-eighth day after admission, however, the use of the salt was resumed; it was taken every four hours for eleven days, when the scalding and discharge were entirely subdued. It should, however, be noticed, that even at this period the urine remained somewhat acid.

Mr. Hilton was led to give the acetate of potash a trial in cases of gonorrhœa from the following considerations. The urethra is in this disease in a highly inflammatory state, and the urine very acid; it is clear that the latter fluid in passing over an inflamed surface must tend to irritate it much, and retard the diminution of the inflammation. The acetate of potash, by its tendency to render the urine alkaline, will therefore remove one of the obstacles which lie in the way of the cure. This same salt has likewise most undoubted depurating properties, and its antiphlogistic powers are very remarkable. Its administration, then, will have the double advantage of rendering the urine less acid and irritating, and of reducing inflammatory action all through the system, and, as a consequence, in the urethral mucous membrane.

[Even where the disease does not yield by the use of the acetate of potash, Mr. Hilton has repeatedly found that, after its administration, a few doses of copaiba completed the cure and prevented relapse.]—*Lancet, Nov.* 2, 1850, *p.* 507.

110.—ON A PECULIAR FORM OF GONORRHŒA.

By W. Colles, Esq., Dublin.

[Mr. Colles remarks how frequently it is that if two cases of gonorrhœa are treated by the same routine of practice, one will be cured in the space of 2 or 3 weeks, while the other will continue for months and even years. Mr. Colles thinks there are various affections or morbid conditions of the neighbouring parts, which, added to the inflammation of the urethra, contribute to keep up the discharge; but he confines himself to one, which he supposes renders the disease much more intractable in its nature and progress, and requires a modification in the treatment, and one which has hitherto attracted little attention. Mr. Colles has no doubt that the peculiar inflammation with secretion of pus from the lining membrane of the urethra, may commence at the orifice, spread along the entire canal, and very often extend to the lining membrane of the bladder itself, and even to the urethra and kidneys, while at the same time a slight cause directs it to the testicles, constituting hernia humoralis. He continues:]

The symptoms are, I believe, never so severe as in that form of disease called catarrh of the bladder, when a thick ropy mucus is secreted in great quantity; they are at times so slight as scarcely to attract the patient's attention, who will merely consider that with him the symptoms are more severe than usual. I have seen this attack of the bladder ushered in with a severe rigor; and on close inquiry we may find that the patient labours under a slight degree of feverishness or uneasiness coming on or increasing towards evening; and that he will complain of a dull heavy pain across the pubis, extending round to the sacrum and anus, and at times even to the region of the kidneys. The calls to pass water will be somewhat more frequent than usual, and when they do occur they are irresistible, attended with considerable pain and forcing, which continue for some time after the last drops have passed away, and which are referred chiefly to the neck of the bladder, and extend from thence to the perineum and anus. The urine, when passed, at times appears clear and natural; in general, however, we can observe a slight cloudiness through it, and on allowing it to settle in a glass vessel, we shall perceive, after one or two hours, a copious yellowish or cream-coloured deposit, consisting entirely of pus. If we take a drop of this urine immediately after it has been passed, before any alteration can occur in it, and place it under a microscope, it will be found to be loaded with pus globules. In some instances in this affection, a number of yellowish, shreddy particles will be seen floating through the urine, which at times alarm the patient, causing him to suppose he is labouring under seminal weakness: on examination, these particles will be found composed of clusters of pus-globules and epithelial scales adhering together.

That the pus thus equally diffused through the urine has its source from the bladder, cannot, I think, admit of a doubt. It cannot be from the urethra alone, for I believe that there is a peculiar action of this canal on its contents, which tends to drive them forwards, and resists any retrograde movement. Besides, the urethra alone could not pour out the quantity of pus we meet with in some of these cases.

My friend, Dr. Fleming, at my request examined the urine with the microscope in some of these cases, and the result of his observations is contained in the following extract from a letter I received from him on the subject:—

"As regards your views respecting those inveterate cases of gonorrhœa, which are so annoying to the patient and so puzzling to the surgeon, no second opinion can be entertained but that the lining membrane of the bladder furnishes a portion of the purulent fluid, and, as you remark, at a much earlier period than at first view would be suspected. To test the direct passage of the pus from the bladder, I have made the following experiments, both on the male and female. In the latter it is often most important to do so. I introduced a catheter, allowed the first ounce or so of fluid to escape, so as to get rid of the urethral discharge, then collected some of the urine in a clean glass, examined it forthwith with the microscope, and found pus globules. I have applied the same test in equivocal cases of hematuria, and found equally satisfactory results as regards blood globules."

In the case which first drew my attention to this subject, the patient a few days after infection had a severe rigor, with considerable pain and irritation of all the urinary organs, attended with a copious deposit of pus, exceeding eight or ten ounces in the twenty-four hours. He soon began to waste, became emaciated, and even symptoms denoting hectic set in; and it was only by great attention he ultimately recovered. I at first feared that an abscess must have burst into the bladder; but his previous good health, and the absence of any symptoms indicative of a collection of matter, soon removed this impression. Since then, having suspected that this purulent state of the urine was at times both a consequence and a cause of the continuance of the disease, I have sought for its presence in several cases of gonorrhœa, and met with it much oftener than I had any reason to suspect; though I cannot form any conclusion as to the comparative frequency of its occurrence. I have observed the urine thus loaded with pus in two or three days after the commencement of the gonorrhœa, and afterwards keep pace with the original disease. I have also in two or three cases known the discharge from the urethra to continue, and pus to be found in variable quantities in the urine, for the space of nearly two years after the original infection.—*Dublin Quarterly Journal of Med. Science, Aug.,* 1850, *p.* 102.

[Mr. Colles offers no specific remedy for the removal of this affection,—we must treat it by the general principles of surgery. When the pus has once appeared in the urine, we must follow the antiphlogistic plan much more strictly than is commonly done; and when the inflammatory symptoms have subsided, we may then resort to those remedies considered as specifics—the balsams or cubebs.]

MIDWIFERY,

AND THE DISEASES OF WOMEN.

111.—ON SEPARATING THE PLACENTA FROM THE UTERUS IN CASES OF PLACENTAL PRESENTATION.

By the REVIEWER of Dr. CHURCHILL'S 'Midwifery,' in the Edinburgh Monthly Journal.

[The largest new dissertations, says the reviewer, which Dr. Churchill has added in this edition, are, on the treatment of unavoidable hemorrhage by detachment of the placenta,—on turning as a substitute for craniotomy,—and on the use of anæsthetics in midwifery.]

If (says the reviewer) there is any truth in the doctrine of the maternal circulation of the blood in the placenta, as laid down by the Hunters (and all modern physiologists admit the soundness and truth of that doctrine), blood *must* escape from the free uterine surface of the placenta when the placenta becomes partially detached from the uterine surface, as in cases of unavoidable hemorrhage; and, consequently, the flooding becomes arrested when the placenta is *entirely detached,*—the plan of treatment which has of late years been proposed to be followed artificially, in instances in which the older modes of treatment were either ineffectual, or specially difficult and dangerous of application, as in dangerous unavoidable hemorrhage, with the os uteri still too rigid to allow of turning; when the mother is too exhausted to permit of turning; when the child is dead or non-viable, &c. Dr. Churchill considers that the hemorrhage in placental presentations does not come from the placenta, but that it escapes from the uterine sinuses, laid bare by the detachment of the placenta. In order to produce the hemorrhage in this way, nature must, at the time of its production, turn back and invert the course of the circulation in these sinuses. But this point we have not time to dwell upon.

Dr. Churchill reasons, and as we conceive, again inaccurately, on the probable number of placental presentations in which some other operation would be required *besides* the detachment of the placenta, provided this plan were adopted in the treatment. In numerous old cases detailed by authors, and in which the placenta was spontaneously expelled, and the hemorrhage ceased, the child, notwithstanding, was afterwards removed by operative interference, as by turning and craniotomy. A great number of these secondary operations were had recourse to merely because the attendant was terrified to risk the non-delivery of his patient, after the placenta was wholly separated. But Dr. Churchill argues, that if we separate the placenta artificially, we will require to adopt these methods of forced delivery, just as frequently as was done formerly under fear of hemorrhage. Certainly not. In placenta prævia, mal-presentations of the child occur more frequently than in natural labours; but transverse mal-presentations, requiring delivery by turning, do not occur oftener than once in twenty cases. And we could point out to Dr. Churchill published cases of placental presentation, where turning, when it was required from mal-presentations, could not be effected till the placenta was removed. Dr. Churchill considers the detachment of the placenta improper in cases where the hemorrhage is very great, but the os uteri still not much dilated. These are certainly, however, the cases best adapted for the practice; and turning in them is frightfully fatal, from the tearing of the cervix uteri (the most vascular part of the whole uterine parietes in placenta-prævia), and the consequent liability to subsequent fatal hemorrhage or phlebitis. Dr. Churchill objects, that where the os uteri is still imperfectly developed, he does not see how the placenta can be easily or safely detached. And yet, in his chapter on the 'Induction of Premature Labour,' he does not see any difficulty in the practice of "separating the membranes for two or three inches around the os uteri," although, at the time of doing so, the os uteri is not in the very least degree opened. If the membranes can be thus separated before the os uteri is at all dilated, the placenta may certainly be equally, or indeed more easily, separated when the os uteri is already partly opened. And in relation to the practice, we are never to forget the true observation of William Hunter, that the placenta is as readily separated from the uterus as the orange is from its rind. After all, the value of the new treatment of unavoidable hemorrhage by complete separation of the placenta, will never be decided by mere theoretical reasoning and objections. Its value, like the value of other new practices, can be decided by the results of practice only. The time has probably not yet arrived when such a decision can be pronounced, because the practice is as yet too recent, and the cases in which it has been followed are yet too few, to permit us to judge definitely of its utility. But the cases and their results, as far as we know them, are in favour of the new practice. We have seen three cases in which the placenta was artificially separated, in accordance with the principles of the new practice; all the three mothers made good recoveries. Dr. Churchill alludes to seventeen cases collected by Dr. West; sixteen out of the seventeen mothers were saved. Dr. Waller, lecturer on midwifery at St. Thomas' Hospital, London, and a gentleman who has had extensive experience in this and other morbid complications, two years ago published a long series of interesting cases of placenta-prævia. In a late addition to his former communications in the 'Medical Times,' he observes, "My confidence in Professor Simpson's plan in these cases, remains unshaken. It will be seen, in reference to my cases, that in nine instances the placenta was separated, either naturally or artificially, before the birth of the child. The same effect was produced in all, namely, *the cessation of the hemorrhage;* and eight of these women recovered. Surely this result could not have been accidental, as some would have us imagine. I am not (Dr. Waller adds) aware that any cases have been recorded wherein hemorrhage has *continued* after complete separation." In these 29 cases of the new practice we have two maternal deaths,—or 1 in 14 died. Out of 182 cases of the same complication, treated by the old plan, and given by Dr. Churchill at p. 388, there occurred 51 maternal deaths, —or nearly 1 in 3½ died.—*Monthly Journal of Med. Science, July,* 1850, *p.* 31.

[For our own part we beg to differ from Dr. Simpson, both as to the propriety of the operation of separating the attachment of the placenta from the cervix uteri in cases of placenta prævia, and as to the reason of the cessation of the hemorrhage. Our objection to this mode of practice is, that although it may be a safe one as regards the mother, it assuredly is a fatal one as regards the child. With regard to the second point, although of slight importance as far as the theory is concerned, it is, nevertheless, of the greatest importance as indicating a most valuable mode of practice to be adopted. We consider that the fact of the flooding ceasing by the method of manipulation introduced by Dr. Simpson, needs no very laboured explanation to account for it, for, we believe, that the separated placenta being still left in situ, acts merely as a mechanical plug upon the orifices of the bleeding vessels, promoting the coagulation of the blood in and around them, and thus effectually presenting a barrier to its further flow. How, then, is this indication to be fulfilled? We answer, simply on the same mechanical principle. If the os uteri is not sufficiently dilated to allow of the operation of turning being performed, our own practice has been for the last twenty years to introduce at once into the vagina sufficient soft linen, lint, or other suitable material as to form an accurate, well adjusted, and effectual plug. By thus filling the vagina no blood is allowed to escape through it, and hence it must accumulate immediately around the bleeding vessels; it cannot force its way into the uterine cavity, the entrance being there prevented by the placenta and the other contents of the uterus Not only does the theory of this mode of treatment sound very plausible, but we have abundantly exemplified its real utility in practice. Indeed, we may even go a step further and explain the use of the plug, on, if we may be allowed to use the term, the hydraulic principle, and this explanation, as will be seen, applies equally well to hemorrhage occurring post-partum. The blood being prevented passing through the vagina, as we have already stated, cannot pass into the uterus, for its contents before delivery will not allow it; but we believe if the uterine cavity was completely emptied, the bleeding vessels being still situated at the cervix, no hemorrhage could take place above the level of their orifices, for as soon as the blood had reached this point, a coagulum would be formed which would cause a cessation of the flooding. It is manifest that the fact of the uterus being found in a relaxed state after labour, and, it may be, completely filled with coagula, is no argument against our theory, because, in these instances, the bleeding must have continued until the blood had reached the surface to which the placenta had been attached, although it may have been even at, or near the, fundus.

We have been in the habit of mentioning this explanation for many years in our lectures on midwifery delivered in the Leeds School of Medicine. Dr. Tyler Smith, indeed, in his article 'On the different Modes of Arresting Uterine Hemorrhage' in 'The Lancet,' Dec. 19th, 1848, and at p. 289, in Vol. XIX of the 'Retrospect,' makes mention of it, but it will be seen that in the pages from 334 to 336 in Vol. XV. of the 'Retrospect,' we had previously distinctly stated this view, in speaking of the treatment of placenta prævia. So far as we remember, Dr. Barnes is the only one besides, who has directed attention to it, and even he merely alludes to it in connexion with a partial presentation of the placenta. His remarks will be found at p. 336 of Vol. XV. of the 'Retrospect.'

The introduction of the plug in the early periods of placenta prævia has many great advantages which the plan of Dr. Simpson does not possess. It is perfectly safe and readily applicable; it promotes uterine action, ensuring the safe dilatation of the os; it preserves the strength of the mother by preventing the serious discharges which would otherwise take place; and lastly, it obviates the necessity. at least as little as possible, of endangering the life of the child. The plug so introduced may be removed every six or eight hours, or oftener, as the practitioner may deem advisable, to allow the evacuation of the contents of the bladder or the rectum, or any examination as to the state of the os to be made. If we find the os then sufficiently dilated, we immediately introduce the hand, separate only as much as is required of the placental attachment to the uterus, rupture the membranes, turn, and so expedite the labour as much as possible.

A long continued series of successful results enables us to speak positively on this subject, and we are quite satisfied, if the practice were always adopted, the cases having an unfavourable termination would be exceedingly few. Out of a very considerable number of cases which have come under our notice where this treatment was followed, certainly not more than two fatal cases have ever occurred.—ED. 'RETROSPECT.']

112.—*New Method of arresting Uterine Hemorrhage.* By M. DIDAY, of Lyons.—[By a similar dilatable caoutchouc tube as that recommended by M. Gariel for epistaxis, M. Diday succeeded in arresting the uterine discharge when all other means had failed.]

The apparatus used consisted of a long tube with a little bladder at the extremity, which was introduced into the vagina, conducted by the index finger, and thus maintained in place whilst insufflation was practised at the other extremity of the tube. The bladder was thus increased in dimensions until it acquired a diameter of about twelve inches, the air being retained by means of a ligature on the free portion of the tube. The hemorrhage was instantaneously arrested; the patient regained strength; and after two days a small quantity of the air was allowed to escape, when no return of the hemorrhage occurring, the entire tube was withdrawn on the day following.—*Dub. Quarterly Journal of Med. Science, Aug.* 1850, *p.* 129.

113.—*On the Use of Turpentine in Uterine Hemorrhage.* By JOHN GRIFFITH, Esq. Communicated by T. T. GRIFFITH, Esq., Wrexham.—[Mr. Griffith makes the following epitome of observations on the use and value of large doses of rectified oil of turpentine, for hemorrhage; not with any claim to discovery, but as calling attention to the great rapidity, power, and certainty of its action in the most formidable cases of uterine hemorrhage the practitioner encounters.]

March 10th, 1847.—Having assiduously, but unavailingly, employed the best remedies known to me in a protracted menorrhagia, half an ounce of oil of turpentine was given, as suggested in 'Blundell's Principles and Practice of Midwifery,' which quickly abated the discharge, restored the pulse, and revived the exhausted patient. In four hours the dose was repeated, with a result the most satisfactory.

Dec. 24th, 1847.—Mrs. J. flooded to an alarming extent after delivery; pulse almost imperceptible; great restlessness; skin cold and clammy; occasional sighing; countenance cadaverous. One ounce of turpentine was given, pressure was made on the uterus, its cavity cleared of coagula, cold wet napkins were applied externally, and yolk of egg with milk was recommended. In a very short time the discharge greatly abated, and the other symptoms disappeared sooner than could have been anticipated. In an hour the woman was able to articulate, and stated that shortly after taking the draught she "felt a sense of warmth all over, and had a hope of recovering, instead of the feeling that she was dying."

April 16th, 1848.—Mrs. S. was brought into a fearfully dangerous state, from partial separation of the placenta, prior to delivery; turning was promptly effected, but followed by terrific loss and prostration; the woman resembled a corpse; it seemed as if a few minutes would terminate her existence. An ounce of turpentine was taken, and repeated in five minutes, followed by the most signally happy result, as rapid as gratifying.

Since March, 1847, eleven instances of excessive and dangerous flooding, prior or subsequent to parturition, and six of menorrhagia, have come under my notice, where the great value of this remedy has been exemplified. Turpentine is not recommended where there is a full pulse, hot skin, with undiminished strength; and when resorted to, other appropriate means should not be disregarded; as abdominal pressure, removing coagula from the uterus, the external application of cold, and not allowing the patient to be moved, &c.

The most convenient way of giving the remedy is giving an ounce of turpentine with half the quantity of oil of sweet almonds, for a draught. This may be repeated in five minutes if the symptoms are urgent. No unpleasant effect has arisen from such a course of treatment, even when the medicine has remained with the patient for thirty hours. Occasionally vomiting occurs, which is salutary.

The *modus operandi* is that of a diffusible stimulus, bringing on rapid contraction of the capillaries, adapting the calibres of vessels to the diminished column of blood, restoring the cutaneous circulation, and producing a comfortable sensation in the place of extreme coldness and death-like exhaustion.

In the cases of menorrhagia, various remedies had been in succession energetically and perseveringly employed without success, such as nitrate of potass, superacetate of lead, sulphate of zinc, infusion of matico, gallic acid, tannin, very large quantities of cold astringent injections; and in the floodings, secale cornutum, galvanism, strong abdominal pressure, plugging the vagina, external cold, &c.

In some of the cases I had the able assistance of Dr. Lingen, who joins in stating that we know no remedy that could be safely substituted for full doses of turpentine in those dangerous cases that so often threaten, and too frequently have proved fatal, notwithstanding the best devised and the most prompt and energetic use of means.

Dr. Lingen's Cases.—Mrs. P., labour a little premature, preceded by hemorrhage, but not from placenta prævia. I was not called to her till she was blanched by loss of blood, and was nearly pulseless. I was obliged to turn with all expedition. This was soon followed by the most alarming exhaustion I ever witnessed, attended with restlessness and a relaxed state of the uterus. A turpentine draught was given, with almost instantaneous effects, namely a glow of warmth, a return of vitality, and a lessening of the discharge, &c., &c. After a few minutes a second draught was given. All hemorrhage now ceased, and she slowly rallied. This case, with a few other particulars, forms the second in Mr. John Griffith's series.

Miss ——, suffered from menorrhagia of a passive and painless kind, that had for the fourth time recurred during the last two years, with a continuance each time of five or six weeks. She was pale, breathless on exertion, had noises in her ears, &c., and was at length compelled to recumbency. All the ordinary remedies, general and local, had been employed, without success. I now gave her a draught, with one ounce of turpentine; this may be said to have ended the matter, though two small doses were afterwards taken, and more than a month has passed without any return of the discharge.—*Prov. Med. Journal, July 24, 1850, p. 400.*

114.—*On Permanent Retention of the Placenta from Morbid Adhesions, with remarks.* By Dr. Robert T. Corbett.—Mrs. S., æt. 30, seven months in her sixth pregnancy, and of debilitated habit, became my patient on the 16th May 1850, when she complained of uneasiness in the belly, of four days' duration, attributed by her to a fright. She said the membranes had ruptured, and that a large discharge of water had taken place. On examination, the os uteri admitted only the point of the finger. I could not tell whether the waters had escaped or not. Bowels regular, from the use of medicine. An opiate was ordered.

17th.—Much the same as yesterday; os uteri as on former examination. Bowels not moved to day. Opiate to be repeated.

18th.—Os uteri dilated to the size of a half-crown piece, and pains decidedly uterine; considerable bloody discharges. As she was a weakly woman—the child undoubtedly small, and supposed to be dead, from motion having discontinued to be felt by the mother, and from the sound of the fœtal heart and placental soufflet having ceased to be heard, to terminate the labour, I gave four half-drachm doses of the ergot of rye in infusion, every twenty minutes, without effect. Habeat enema domesticum quam primum.

19th.—Bowels opened from enema; had slept very well; os uteri a little more dilated; still considerable oozing. Pains those of labour, and stronger than yesterday. I administered the ergot in the same doses as the day before, but the drug was of a fresher quality. The pains were very much increased in power and frequency by its exhibition; some were even considerably protrusive, but nature had not the power, and the effects of the drug wore off. I then administered an opiate, to quiet the system and procure sleep.

20th.—Patient had had a few hours' refreshing sleep during the night, and this morning at half-past nine o'clock, the fœtus was expelled, with little suffering, and slight labour pains. I was sent for. On my arrival I found the fœtus expelled, and in a putrid state. The placenta was still in the uterus, which was flaccid. The cord came away with the very slightest traction. After dashing whisky over the abdomen, grasping the uterus tightly, and trying the usual means to make it contract, and persevering for half an hour, as there appeared to be very little if any contraction, the left hand was completely introduced into its cavity, when the membranes were found to adhere all around, so that I could not positively say that I felt the border of the placenta, although I thought so, from my fingers passing over a rounded ridge. The usual plan of separating it, by insinuating the fingers between it and the uterine surface, and gently moving the hand from side to side, with a sawing motion, being out of the question, the plan of Hamilton and Burns was tried, without effect. The uterus contracted powerfully upon the hand, but became quite flaccid as soon as it was withdrawn. My manipulations were continued for about forty minutes. A bandage, with a compress, was now tightly applied, a grain of opium given, and the patient left to rest. The patient progressed as well as is usual in an ordinary convalescence after delivery, till the evening of the 25th, when, after the loss of not more than 12 or 14 oz. of blood by hemorrhage from the uterus, she died, notwithstanding the free exhibition of stimuli; in short she never rallied from the collapsed state, which immediately followed the discharge, and died in four hours. As I became aware of the intimate adhesion between the placenta and the uterus at my first attempt at extraction, and as in such cases I do not approve of any after attempts at removal, no further trial to separate it was made.

Inspection 60 Hours after Death.—Three medical gentlemen besides myself were present. Upon opening the cavity of the abdomen, the uterus was seen 6½ inches long by 4½ broad, having a baggy empty feel, but presenting a healthy surface. The uterus was detached from its position, and an incision made longitudinally through its anterior wall, when the placental mass and membranes were seen to fill its cavity. The inner surface of the uterus presented a dirty green or blackish hue, and after having been repeatedly and carefully washed, was found to be wholly covered by a lining presenting a granular fleshy appearance, at least ⅛ of an inch in thickness, in some places even more. This membrane was, with some difficulty, separated, to a trifling extent, from what appeared to be the disorganised lining of the uterus. The placenta adhered to the superior and anterior part of the uterus, rather to the left side, which was the spot where I had felt it. The adhesion was complete, and so perfect that the placenta could not be detached without destroying its own substance, or that of the uterus. We could not discover the spot from which hemorrhage had taken place. The structure of the placenta resembled that of a hepatized lung, but was more fibrous, and was so intimately connected with the parietes of the uterus, as to render it impossible to tell where the placenta terminated, and the uterus commenced. The fœtal membranes were also very extensively and intimately attached to the walls of the uterus; indeed so intimately, that the attachments could be broken only with great force, and would of themselves have been a cause of retention of the placental mass.

[In conclusion, Dr. Corbett remarks,—]

1st, That this case proves the truth of the generally received opinion, that all efforts to extract the placenta may be unavailing.

2d, That it strengthens the opinion "that constitutional predisposition is a more frequent cause of morbid adhesion than any other."

3d, That it proves that morbid adhesion of the placenta is produced by the formation of a new membrane, the result of increased action in the lining membrane of the uterus, and by the peculiar character of the blood in pregnancy.

4th, That it proves that agglutination of the fœtal membranes to the uterus, of itself may be a cause, if not of permanent retention of the placenta, at least of retention of it, until putrefaction shall so have destroyed their texture, as to permit of the mass falling away; although, from the nature of the disease, such a case must very seldom occur.—*Monthly Journal of Med. Science, Nov.,* 1850, *p.* 410.

115.—*On Menstruation in Relation to Pregnancy.* By Prof. DUBOIS. – Conception (says the professor) may take place in a woman not yet arrived at the age at which she ought to menstruate. There are, in fact, many women who do not menstruate till the seventeenth, eighteenth, or nineteenth year. Such women may become pregnant at that time of life, although they have never menstruated. M. Dubois has seen a woman become pregnant two years after the cessation of the menses. The woman, finding her abdomen enlarging, entered the medical department of an hospital. The physician under whose charge she was, had so little idea of the woman's being pregnant, that he delivered a clinical lecture on the case, as one of ovarian dropsy. On making an examination of the case, M. Dubois easily made out the pulsation of the fœtal heart. In fact, labour very soon supervened.

The menses may be suppressed physiologically, and yet pregnancy take place. Thus, it is not rare to see nurses become pregnant before menstruation has re-appeared. We find also, that women who are extremely irregular, who, for example, menstruate only once or twice a year, become pregnant; although, in general, this state is one very unfavourable for conception.

Various diseases and changes in habits may derange the menstruation, and give rise to the idea of pregnancy. This error occurs frequently, chiefly to persons anxious to become in the family way. Nothing is more common than to find the menses suppressed for some time after marriage. It is also very frequently observed, that women leaving the country to reside in the town, suffer from suppression. This may be said to occur habitually in young women coming from the country into domestic service in Paris.

In other cases, the menses, after having been suppressed for three or four months, re-appear suddenly, with some profuseness. This is sometimes taken for the occurrence of abortion, when it is merely the recurrence of the menses after they have been suppressed, in consequence of some change in the habits of the female.—*Journal de Méd.*—*Monthly Journal of Med. Science, July,* 1850, *p.* 73.

116.—PROLAPSUS OF THE PREGNANT UTERUS.

By Dr. Merriman.

[The following is extracted from a letter to the Editor of the 'Medical Times':—]

In Professor Rizzoli's case, the procidentia had, as usual, been temporarily cured by the enlargement of the uterus, causing it to rise into the abdomen; but of course, when labour pains came on, the uterus was again drawn down, even to the extent of being propelled beyond the vulva—an occurrence which several cases on record have demonstrated to happen occasionally. It seems fair to suppose that the first pain produced this occurrence, combined as it was with the "raising a great burthen;" but we are not clearly informed whether any part of the uterus itself was propelled beyond the vulva, except by inference from the tearing of the perineum, and "a body" being felt to pass. How the perineum can have been torn at this time, unless the uterus with its contents had passed the os externum, I do not understand; yet it seems to have been found ruptured by the accoucheur on his arrival, before he had recourse to instruments. We are told that "a small foramen allowed a finger to pass up to the head itself." Of course the part most protruded would be the vagina; but we are not told that the os uteri was felt at all; are we then to understand that the os was fully dilated? That seems improbable; but if the practitioner had taken care to observe the facts, he would not have been so much at a loss, as he appears to have been, what treatment he should pursue. Again, we are told that attempts at reduction were vain, from the recurrence of pains; and that after some time mortification appeared imminent, from the amount of congestion observed in the protruded vagina. But to what is this congestion to be attributed? Was it not from the pressure of the distended uterus upon the veins of the displaced vagina preventing the return of blood from it? If so, a constant firm support from the hand of the attending practitioner from the first would surely have prevented it; besides that, more than this, it would have enabled the contractions of the uterus to propel the head of the child against the os, and thus to dilate it. This, however, could not happen if no counter-pressure was exercised against the bearing down of the pains.

Medical practitioners of the present day seem too little disposed to look back and learn what others have done in times past; yet there are on record several cases of *procidentia vaginæ et uteri*, at the commencement of labour, which well deserve to be consulted and borne in mind. Let me refer to a few of these. In a case recorded by Mr. James Shaw, in the 'Memoirs of the Medical Society of London,' (Vol. I., p. 213, A.D. 1786,) when mortification appeared commencing, the perineum was found, on examination, to be dilatable; it was, therefore, dilated by the finger; the protruded mass then slipped back; and the patient fell asleep for two hours; after which, pains again came on, and a fine boy (her fifth child) was born by the natural efforts. The woman bore subsequently two children, and never afterwards complained of more than a slight bearing down.

In the 'Medical Museum,' (Vol. I., A.D. 1763,) Mr. Antrobus, of Liverpool, describes the case of a poor woman, who conceived of her fourth child while labouring under a "prolapsus uteri." She went her full time, although the cervix became "scirrhous" from exposure, and, at the time of labour, the first pain drew up the uterus, but the waters afterwards breaking, it was propelled down again before the head. Counter-pressure was therefore made, and the part lubricated; the child was born, and the "prolapsion" replaced. The woman did perfectly well.

Mauriceau gives the following case (Obs. 6, A.D. 1669):—The uterus had protruded during labour to the extent of half a foot, and was as big again as the head of an infant. The os looked like a phymosis, of which the lips were three fingers' breadth thick, and quite livid. He introduced his hand by degrees into the uterus, and thus guided the head in its exit. He subsequently applied a pessary.

Portal (Obs. 10, A.D. 1666) describes a case of protruded uterus in a first pregnancy, where the orifice was only half a line across. After consulting with M. Moreau, he introduced very carefully a silver sound; by this means he was enabled to insert his finger, and so dilate the orifice still more, it being, after three hours' dilatation, the size of an "*écu blanc*." After an hour's cessation, the dilatation was renewed, by both hands being carefully insinuated; the bag of waters assisted, and two pains brought the child into the world. The uterus was replaced, and all did well. The woman, previously to her marriage, had always had a "relaxation," but could easily replace the womb. During her pregnancy, however, it ceased to come down till the labour set in. In several respects this resembles the case recorded in the 'Medical Times.'

Smellie (Collection XVII., No. I., Case 2, A.D. 1746) describes a third labour, in which the os uteri was pushed down by the pains beyond the os externum:—"During every pain I kept up the child's head, and the mouth of the womb, which I gradually dilated with my finger, till, being fully opened, it easily slipt all round the head, and this afterwards opening the os externum by degrees, was safely delivered." Perfect (Case 77, A.D. 1766) relates a case in which there was "an entire hernia of the uterus," yet the os uteri was "so high up, that he could but just reach it with his finger in the vagina." "He gradually distended the os tincæ," introduced his hand, and, finding the feet presenting, seized them, and brought away the child.

The case related below has never, I believe, been given to the public. I have, therefore the more pleasure in introducing it here, as it is an excellent example of this class of labours. The case occurred several years ago in the practice of Mr. Gristock, surgeon, of Wardour Street, Soho. Mrs. Purdie, the wife of a journeyman gunmaker, in Little Pulteney Street, a delicate woman, had already been the mother of two children. After the birth of the second, she was much troubled with a sensation of bearing down, and sometimes of a slight protrusion of the uterus, which she endeavoured, very ineffectually, to remedy, by wearing some kind of bandage. The protrusion became greater when she next became pregnant, but diminished, and was at length wholly removed, as the uterus enlarged from gestation. When, however, the labour drew near, she was again sensible of much weakness and bearing down; and at length when the pains actually came on, she was much alarmed by finding that a large fleshy mass had passed through the os externum with a good deal of fluid discharge. She sent for Mr. Gristock, who found that not only a considerable portion of the vagina was protruded, but that the os uteri was to be felt considerably without the os externum. He ordered her to bed, had the parts freely fomented, and requested a friend to visit the patient with him. They found her much alarmed, but presenting no symptoms indicative of danger. It was, therefore, determined not at present to employ any active interference. The pains at first were not strong, but as they increased in strength, it was thought advisable to expand the hand over the protruded part, so as to make the ends of the fingers afford five points of support to the prolapsed vagina. By degrees it appeared to recede during the pains, and, at last, during a very strong pain, the whole mass slipped up into the pelvic cavity; the infant was speedily born, the placenta came away, and the uterus contracted firmly, remaining in its proper situation. This woman got quite well, and in less than two years became pregnant again; she then came under the care of Dr. Hugh Ley. The same procedentia again took place, and was managed in the same way, with the same happy result. After some time she became pregnant again, but on this occasion there was no prolapsus; the child was found to present by the arm, and was turned by Dr. Ley, but was still-born. A very interesting question may be asked here, namely, Did the transverse position of the child on this occasion prevent the ready subsidence of the uterus into the pelvis? I have been unable to obtain any further account of this person after this last confinement.

I cannot help regretting that the accoucheur in the case recorded by Professor Rizzoli did not practice the same form of treatment which has proved so effectual in the hands of the above-named experienced practitioners. He would then, possibly, have saved the life of the child, and have avoided the lacerations of the vagina and consequent hemorrhage recorded. Let me merely add, that I have presumed, in this criticism, that the rupture of the perineum took place before the medical man arrived; but if we suppose that, as in Mr. Shaw's case, the uterus descended without tearing the perineum, the employment of the forceps would be almost certain to produce that accident.—*Med. Times, April* 7, 1850.—*Monthly Journal of Med. Science, Nov.*, 1850, *p.* 474.

117.—*Successful Case of Extraction of the Child by the Cæsarean Section, after the Death of the Mother.* By GEORGE HARLEY, Esq., House-Surgeon to the Edinburgh Maternity Hospital. (Read by Dr. Simpson, on 5th June, at the Meeting of the Edinburgh Medico-Chirurgical Society.)—[The mother in the case, Catherine Davidson, aged 39, unmarried, was admitted into the hospital, being in the seventh month of her second pregnancy. She stated she had been very delicate during her pregnancy; complained of shortness of breathing in ascending stairs, and of dyspepsia. As her legs were swollen, and her face pale and puffed, her urine was tested, but no albumen was found present. On the 25th of April, 1850, nearly a month before her expected confinement, Mr. Harley was summoned to her assistance, the matron stating her to be in a state of suffocation. Mr. Harley, on reaching her, found her with all the symptoms of approaching asphyxia, and the countenance denoting intense anxiety. He immediately ordered her dress to be loosened, and her stays torn off; as soon as she could be placed in her bed, an emetic of ʒss. of sulphate of zinc was administered, but the symptoms becoming gradually more urgent, laryngotomy was performed. There being no tracheotomy tube in the house, Mr. Harley cut about 3½ inches off a full-sized flexible male catheter, and introduced it; but as the mucus did not readily come through it, it was withdrawn. The emetic was now repeated, and in less than five minutes above eight ounces of white frothy matter was expectorated. As the symptoms still increased, Mr. Harley proceeded to perform the operation of tracheotomy; but before the operation was much more than half finished, all attempts at respiration ceased. Mr. Harley continues:]

All hope of saving the woman being lost, my next thought was to save the life of the child; so I again snatched up the bistoury, ripped down the patient's dress, and instantly made an incision in the mesial line, through the parietes of the abdomen, commencing a little above the symphysis pubes, keeping close to the right side of the umbilicus, and terminating a little above it. The uterus then appeared, and I proceeded to make careful incisions in it, to avoid wounding the child. In making these incisions none of the intestines came in the way of the knife, and there was very little or no bleeding from the wounds.

When the cavity of the uterus was reached, the liquor amnii escaped. I put the two first fingers of my left hand into it, laid the back of the knife against them, and cut downwards and outwards. One side of the nates now appeared. I then put the right hand into the uterus, caught hold of the first thing that came in my way, which happened to be a leg, and withdrew the child without any difficulty, the uterus not contracting round the neck.

The child, on extraction, looked beautiful and clean, as if it had been carefully washed; it was to all appearance quite dead, no pulsation being felt, either at heart or cord. I dashed cold water on its chest, gave it a rub, and then put my mouth to its mouth, depressed and pushed back the larynx, held the nostrils with the one hand, and pressed on the chest with the other, after each time that I filled the child's lungs with air.

After a few minutes I stopped to take breath, and during that time I applied friction and aqua ammoniæ to the breast, and on using artificial respiration for some minutes more, the child's heart began to beat, and the pulsations in the cord became distinctly visible; a ligature having been put upon it, the child was cut away. It was a male, weighing 6 lb. 12 oz., and measuring 18½ inches. Shortly afterwards I put my hand into the uterus, and peeled the placenta from the back part and right side of the organ, as it would not come away by pulling at the cord; it weighed 1 ℔. 4 oz., and the cord measured 20 inches. The wound was stitched up, and the body removed into the delivery room, where the post-mortem examination took place.

Not more than twenty minutes elapsed from the time the patient was seized in the kitchen till all was over.—*Monthly Journal of Medical Science, July*, 1850, *p.* 21.

118.—*Turning Successful in a Case of Fibrous Uterine Tumour, occupying the Centre Cavity of the Pelvis, and subsequent Expulsion of the Tumour.* By D. R. Rankin, Esq., Carluke.—[The attention of the profession having been lately directed by Dr. Simpson (See Retrospect, vol. xvii., p. 308) to the question of turning in certain cases of small pelvis, to avert the destruction of the child by craniotomy, the following question is asked, "Does experience teach that the base of the head offers less resistance than the vertex in passing through the outlet of the pelvis?" The case of Mr. Rankin will illustrate the point.]

Mrs. G. is 27 years of age, and the mother of five children. The first two births were natural; but in the third (24th August 1846) some difficulty, I learn, arose in the early stage, to the probable cause of which her subsequent history points.

At the fourth birth (31st March 1848), the difficulty became too apparent. After watching the efforts of nature for thirty-six hours, and finding that though the head had entered the brim of the pelvis, an insuperable obstacle hindered its further progress, Dr. Selkirk, whose case it was, requested my advice and assistance.

A large tumour was found to occupy the right hypogastric region, producing displacement of the uterus; and, on examination, the vagina was found to be depressed, as well as compressed, and thrown upon the left side of the pelvis by the tumour, which thus occupied the greater part of that cavity. By the ordinary mode of examination, the uterus could not be reached; but on introducing the hand, the head was found free of the uterus, although completely retarded from farther progress by the counteraction of the impending mass.

As there seemed no alternative, perforation was resolved upon, which, from the little mobility and incompressibility of the tumour, was not easily accomplished; but the ultimate extraction of the child was effected, after tedious, difficult, and careful manipulation with the crotchet.

When a subsequent pregnancy came to the knowledge of the medical attendant of the family, he naturally felt anxious. Encouraged in the attempt, the induction of premature labour was tried by him in the seventh month; but it could not be accomplished, the uterus being displaced, and beyond reach.

On the 1st of February, and again after an interval of some days, I examined Mrs. G., with the aim of determining how her case was to be met when the urgent hour arrived. The prospect was most unpromising. The recollection of former difficulties, with the certain knowledge that the obstructing agent was more formidable than before, made me look forward to the crisis with a feeling of awe. To lessen the burden of responsibility, and in order that the patient might have the advantage of the best advice, Dr. Wilson (of Glasgow) was consulted on the 9th, and through him was secured the advice of Drs. Watson and Macfarlane.

All concurred in viewing the case as one of great difficulty, involving the probability that the Cæsarean operation might, of necessity, be resorted to, should turning fail. Efforts were made to secure the consent of the patient to remain in Glasgow, under Dr. Wilson's care; but she declined, and reluctantly I had to undertake the case.

On the 25th February, labour pains came on. Dr. Selkirk had been in attendance about twelve hours, during which time the pains were not urgent. In a note I had from him that night, at nine o'clock, after intimating the onset of true pains, he stated,—"The tumour feels as hard and unyielding as ever. The passage of the child will prove an impossibility." On examination, two hours afterwards, the tumour was found to occupy the entire cavity of the pelvis, and was acted upon and pressed down by every effort of the womb. The presenting parts could not be reached by the ordinary mode; but on introducing the hand, to ascertain how far the tumour could be displaced upward, it was found obedient to pressure to some extent; and by passing the hand onward, on the left side of the pelvic cavity, against which the vagina was forced, a small bag of the protruding membranes was felt; but, unwilling to rupture them at this stage, and as it was otherwise impossible to reach the womb, I resolved, in absence of any urgent symptom, to wait till the distended membranes exercised somewhat longer their relaxing influence. At half-past one o'clock on the following morning I introduced my hand, moulded the passage by acting on the tumour through the walls of the vagina, and passed the hand up carefully till I became sensible of the presenting parts, through the well-dilated orifice, which was situated above the edge of the left ilium. The liquor amnii was now evacuated; the hand passed into the womb, and what proved to be the left foot was secured. Very considerable force was necessary to bring the child into the pelvis. During the operation, the tumour was gradually forced up from the cavity of the pelvis into the cavity of the abdomen, and a sensation of tearing was communicated to the hand, and rendered audible, which led me to conclude that some fearful disaster was impending. This was soon found to be referable to the parts of the mother; for the child was delivered uninjured, though in a state of asphyxia, from which it rallied within ten minutes.

The placenta was thrown off within fifteen minutes, the uterus contracting regularly and satisfactorily above and partially behind the tumour.

A train of symptoms, by no means promising, followed. There was acute pain in the uterine region for several days, much sleeplessness, and occasional delirium A fetid discharge followed, and a smart attack of erysipelas of the face and head came on. On examination on the seventh day after delivery, the maternal canals over the most prominent part of the tumour were found to be in a sloughy condition. A few days after, a small projecting mass was found in the seat of the slough, and fetid parts constantly escaped, the nature of which was not ascertained. On 3d April—the thirty-eighth day after delivery—the patient informed me that something protruded from the vagina; and with little difficulty, the parts being all but free, I extracted a partially putrid mass, of flattish form, ten inches long, five broad, and weighing fourteen ounces,—consisting of a dense fibrous substance, fringed here and there with small cells, emptied of their contents.—*Monthly Journal of Med. Science, July*, 1850, *p.* 13.

[We think this practice of turning, in cases of deformed pelvis, as illustrated by the preceding case of Mr. Rankin, strongly corroborated by some remarks made by the reviewer of Dr. Churchill's Midwifery in the 'Edinburgh Monthly Journal' for July, 1850, p. 34. Dr. Churchill is said to quite misunderstand Dr. Simpson's principles of practice, when he fears, that in some cases, we might turn the child and yet find the pelvis too small to let it pass without craniotomy. The reviewer says:]

Among Dr. Collins' 16,000 cases in the Dublin Hospital, there was no pelvis too small, for there was none below 2½ inches in its conjugate diameter, and only one so small even as this; and we have seen a full-sized child pass through as small a pelvis by turning, its head becoming compressed during its passage. Dr. Churchill objects that, for the success of turning in contracted pelvis, we must be able to ascertain the "exact diameters" of the pelvis. He might as reasonably argue that, for the success of craniotomy in contracted pelvis, we must be able to ascertain the "exact diameters" of the pelvis. The general case is this, —the head becomes arrested at or above the brim, in consequence of the head being slightly too large, or the brim too small; and the question is, how is delivery to be effected? Take a steel perforator (says Dr. Churchill), introduce it through the cranium of the child, allow some of the brain to escape, so as to diminish the head, and thus allow of its passage. We say, no; if the child is alive, and there is no contra-indication, extract the infant by turning, and in its extraction subject the flexible skull of the child to transient, and not necessarily fatal, compression, rather than to fatal perforation. He fears that, even if the child were turned, we might sometimes still require to reduce its head by perforation after all. But if so the operation is more simple than if the arch of the cranium presented, in so far that no crotchet is then required. He objects further that the operation would be of "very little benefit to the child." Certainly we admit that *all* the benefit it receives is giving it life, or a good chance of life, instead of being submitted to certain death, by having its head perforated. Numerous children's lives have been already saved by the practice. And we feel fully assured of one fact, that in proper cases the operation of turning gives the mother a far better change of escape; turning, according to Dr. Churchill himself, in other parts of his work, being an operation fatal in about one in every sixteen cases to the mother, whilst, by his own showing, craniotomy is fatal in about one in every five cases to the mother, and, of course, in every case fatal to the child.—*Monthly Journal of Med. Science, July,* 1850, *p.* 34.

119.—*Remarks on the Structure of the Neck of the Womb, and on one of the numerous forms of Disease to which it is liable.*—By Dr. Edward J. Tilt.—[Dr. Tilt describes this form of disease, which he has met with, and which he supposes arises from a form of inflammation of the neck of the womb, as an erectile swelling on that part. Making a few observations on the anatomy of the neck of the womb, which he states is still imperfect, Dr. Tilt says, with regard to the muscular fibres, that their disposition and proportion have not yet been thoroughly investigated, but that it is admitted by Theller, Burns, Negrier, and many others, that they exist in smallest proportion in the surgical neck of the womb. He then proceeds:]

As the erectile structure of the os uteri was not acknowledged by Dr. Snow Beck, I requested Mr. Quekett to give me the benefit of his great experience in microscopical investigations, and we examined the disposition of the minute vessels in three specimens for which I am indebted to my friend Dr. Jenner. One of the specimens was removed from a middle-aged woman, who had borne children, another from a girl of sixteen years of age. On examining perpendicular sections of the lips of the os uteri of both subjects, Mr. Quekett showed me distinctly the looping of the blood-vessels, their varicose distention in certain points, and more particularly at the branching off of blood-vessels one from another—a disposition of blood-vessels which is characteristic of erectile tissue, as it exists in the lips or the nipple. The third specimen was taken from an old woman; the os uteri was so completely effaced, that although there was a very small aperture communicating with the womb, this aperture was not surrounded by any lips. On examining, however, sections of tissue removed from the vicinity of this aperture, we could not distinguish any varicose disposition of the minute vessels. Neither could we do so in a fourth specimen, exactly similar to the preceding one, which was sent to me by Dr. Allen, and which I lately examined microscopically with Dr. Hassall.

These researches tend to prove that during the reproductive portion of the lifetime of woman the lips of the womb are permeated by minute blood-vessels, which are similar in disposition and structure to those of the nipple and other erectile tissues; but further investigations and careful injections are necessary to give certainty to what I now offer as probable.

On passing the uterine sound, I have sometimes seen the lips of the os uteri enlarge and become more turgid; but knowing how easy it is to see what we suppose we ought to see, I should not have mentioned the fact if Dr. Tyler Smith had not assured me that he had seen the same.

Supposing the erectile disposition of vessels in the neck of the womb to be fully confirmed, the morbid development of that disposition would well account for a form of uterine disease, which has been described by Dr. Ashwell, and which Duparque has confounded with others under the name of "engorgements congestifs du col," and which Professor Recamier has long ago called "erectile swellings of the womb," having been led to adopt that denomination, not on account of any anatomical researches, but by the peculiar characteristics of such tumours. Whatever may be the explanation of that pathological state of the os uteri to which I at present direct attention, I contend for the fact of its not unfrequent existence; and the following case is an example of the rest :—

Case.—A few months since I was consulted by a lady, aged twenty-five, who had menstruated for the first time at the age of fourteen years, and who did so regularly till she married, three years ago. Since then she had suffered from dysmenorrhœa, and latterly from backache and leucorrhœa. After adopting the usually employed method of treatment by injections and tonics for three weeks, I made an examination, and found the vagina hotter and more irritable than usual; and on applying the speculum, its field was filled by a well-circumscribed substance of a bright red colour, and yielding to pressure. On enlarging the aperture of the speculum (Coxeter's), it was easy to see that the whole mass was formed by a swollen lip of the os uteri, which on being carefully dilated, did not present any erosion or ulceration, either on its external portion or in its cavity. The internal mucous membrane was redder than usual, and there was an increased flow of the normal glutinous secretion. I scarified the swollen surface, and ordered tepid injections, mild purgatives, and repose. A few days after the tumour was smaller, more livid, less painful, less tense on pressure, and I whitened it by rapidly passing the nitrate of silver over its surface. I ordered injections with a cold solution of alum. I repeated the application of the nitrate of silver every five days for a month; and finding that the swelling did but slowly decrease, I applied iodide of iron to it every four or five days with a large camel-hair pencil, and in three weeks the swelling disappeared. The patient was under treatment for three months; and never during repeated speculum examinations could I find any other lesion of the neck of the womb, except an increased intensity of colour.

Sometimes both lips are swollen, but generally only one is so. Out of twenty well-marked cases of this description which I have observed, in two the swelling was general; in eight, the posterior lip was swollen; and in ten the anterior. When one lip is swollen the shape which it assumes puts one more in mind of the rump of a fowl than of anything else. Most of my twenty patients were of a lymphatico-sanguine temperament; they were all married, and in five the complaint came on within the six months which followed marriage. In almost all the cases intercourse was painful. Sixteen of the cases were dispensary patients; the other four were ladies refractory to the exigencies of judicious treatment.

These details lead me to infer that this particular form of uterine disease is principally caused by matrimonial intercourse, and by the necessity in which the poor are placed, of standing about for hours after they have been more or less severely cauterized. This influence of long standing after cauterization, is borne out, so far as my experience goes, by the fact of the same thing occurring in women moving in a higher sphere, and who, very soon after cauterization, are found dancing till two or three o'clock in the morning.

That this form of disease may be attended by, and caused by, erosion of deeply-seated ulcerations of the cervix I fully believe; but that it does not always depend upon ulceration is proved by the fact, that in twelve out of my twenty cases I could not detect any solution of continuity of the uterine mucous membrane.

I have already pointed out the treatment I employ in such cases. When the tumour is hot and tense, and red, leeching and scarifications must be had recourse to. I prefer to scarify the tumour, or else to apply two or three leeches to the vagina in its vicinity, in order to disgorge the tumour without irritating it by the leech-bites. These cases are obstinate on account of the recurrence of the causes alluded to; and it is better to ring the changes on the different styptics we possess, than to persevere in the use of one. I never employ potassa fusa in this form of uterine disease; I principally make use of nitrate of silver, and I have, with advantage, alternated its use with that of the solid sulphate of copper, or a strong solution of diacetate of lead, or of the iodide of iron.

I deny the propriety of leaving such cases to themselves; sometimes, no doubt, they would be dispersed by the powers of nature, but in most instances the hypertrophy increases, and by extending to the rest of the neck, produces those uterine displacements which have lately been studied too much without reference to their real cause. The patient cannot be kept to herself, because under increased fatigue of the sexual organs, or of the general system, a greater amount of irritation sets in, and the passage of a leucorrhœal discharge on an inflamed surface produces granulations, excoriations, and ulcerations, where primitively none might have existed.—*Lancet, August* 10, 1850, *p.* 173.

120.—ON THE USE OF THE SPECULUM.

By Dr. Robert Lee, F.R.S., &c.

[Dr. Lee in the following article states concisely the results of his observations during the last twenty-three years, on the use of the speculum in the diagnosis and treatment of uterine diseases. He says:]

In the first great class of organic uterine diseases, which comprehends fibrous, fibro-cystic, glandular, and all other tumours which are not malignant, I have derived little or no aid from the speculum in their diagnosis or treatment. When fibrous or other tumours are formed under the peritoneum, or between the muscular fibres, or under the lining membrane and distend the cavity, their existence can only be determined by a careful examination of the hypogastrium, and of the interior of the pelvis through the vagina and rectum. The uterus is usually felt large, hard, irregular, and the cervix shortened. Where these tumours have passed partially or completely through the os uteri, their size, density, the length and thickness of their roots, and the relations these bear to the os and the cervix uteri, can only be determined by the touch. I have never detected a small polypus within the os uteri, or hanging through it, which I had failed to detect with the finger. In cases of this description I have, however, repeatedly employed the speculum to ascertain the colour of the polypus, and the degree of vascularity of the investing membrane, which without ocular examination could not have been determined. The knowledge thus acquired was of no use in the treatment. In a case of fibro-cystic polypus of the uterus, which occurred at St. George's Hospital upwards of eight years ago, under the care of Mr. Cutler, the speculum was introduced, and we saw clearly the small cysts under the vascular covering membrane. The speculum was withdrawn, before the operation for the removal of the tumour had commenced. In a case of small glandular polypus in a sterile married lady, which I saw with Mr. Painter, the speculum was employed, and it made us acquainted with the colour, and more perfectly with the nature and diminutive size of the disease. The polypus was removed with the forceps, after the speculum had been withdrawn. In a similar case which I saw with Mr. Jonson, of Grosvenor Place, the speculum was employed for the same purpose after the nature of the disease had been satisfactorily ascertained by the touch. The tumour was readily removed with the forceps after the speculum had been withdrawn. Very recently I saw a sterile married lady, with Dr. Meryon, who had a small polypus hanging through the os uteri. In that case the speculum had been employed before the patient came to London, and for that reason alone I had recourse to it. The tumour presented the appearance of a large bean, was of a bright-red colour, like vascular tumours of the meatus urinarius, and bled freely when touched, though the surface was not ulcerated. The length and thickness of the root on which the speculum threw no light, had previously been ascertained with the finger. The forceps was passed through the tube, and the tumour removed; but it is now my conviction, that the peduncle would have been more effectually destroyed, had the forceps been passed up along the fore and middle fingers of the left hand in the usual manner. More recently, with Mr. Henry Charles Johnson, I removed a small polypus, hanging through the os uteri, with the forceps, in a most satisfactory manner, without the speculum altogether. In all large uterine polypi it is obvious that the speculum can be of no use, and that it would not enable us in any case to decide whether a tumour in the vagina was a polypus or an inverted uterus, a small portion only of the lining membrane of the uterus, in either case, being all that could possibly be presented to the eye. A case of large globular tumour in the vagina, now under the care of Mr. Cathrow, strikingly illustrates the truth of what has now been stated respecting the use of the speculum in the diagnosis of inverted uterus, and fibrous tumours.

In all the varieties of malignant disease of the uterus, scirrhus, fungoid cancer, and corroding or phagedenic ulceration, the speculum has given me no assistance whatever in their diagnosis and treatment, either in the early or in the advanced stages. I have never, in a single instance, failed to determine by the sense of touch, when cancer of the uterus had commenced; but I have repeatedly, after the most careful examination, both with and without the speculum, suspected that it would be developed, when the result proved that my fears were groundless. I am fully satisfied that the speculum does not enable us to decide earlier than the finger that cancer has commenced; and if it did so, as some maintain, and enable us to make applications to the os uteri, which could not be made without, not the slightest advantage would be gained in practice. When cancer of the uterus has advanced to ulceration, the speculum is not only useless, but positively injurious, and ought not to be used. In the year 1827, when I first became acquainted with the speculum, and saw it very frequently employed in a great public institution, a patient with ulcerated carcinoma speedily died from hemorrhage after the introduction of the bivalve speculum. In cases of ulcerated cancer of the uterus, the best French writers have interdicted its use. "L'état de la matrice elle-même," observes M. Teallier, "interdit quelquefois l'usage du spéculum; les ulcérations saignantes, et profondes du col, son énorme développement, les fongosités qui s'élèvent de sa surface empêchent et rendent même inutile ce moyen d'exploration." M. Pauly gives the same opinion, and relates a case in which the speculum produced extensive laceration of the vagina, and death in two hours

Several cases of ulcerated carcinoma have come under my observation, in which the speculum and ignorance of uterine pathology appeared to have led to the commission of the most grievous mistakes. In one of these, even in the last stage, the speculum and caustic had been employed almost daily for months, and hopes held out of recovery, when the patient had only a very short time to live. In another case which I saw with Mr. York, where the os and cervix uteri, and a portion of the vagina, were all extensively disorganized by cancerous ulceration, the speculum and caustic were used, at first daily, and then twice a week for months, by the physician under whose care she came at last, without the slightest benefit. I have seen other cases analogous to these, and several others have been related to me which I had not seen.

From the age of maturity to the middle period of life, the uterus is rarely, seldom at least, comparatively with advanced age, affected with organic disease of any kind. Amenorrhœa, hysteria, dysmenorrhœa, menorrhagia, leucorrhœa, and various nervous affections local and constitutional, are those from which females chiefly suffer before the age of twenty-five or thirty. An examination of the physical condition of the uterus in unmarried women, either with or without the speculum, I have always refused to make, even when requested to do so, unless pain severe and almost constant in the region of the uterus existed, leucorrhœa or hemorrhage, which did not yield to treatment, and where the symptoms did not make me strongly suspect the presence of some displacement or organic disease. In unmarried women, whatever their rank or condition in life may be, the integrity of their structures should not be destroyed by the speculum, nor their modesty wounded by an examination of any kind without a necessity for such a proceeding being clearly shown. Even in married women, who are barren, or who have had children, it is unjustifiable on the grounds of propriety and morality, to institute an examination of any sort, unless the symptoms warrant the supposition, that the uterus is displaced, or is in a morbid condition, the nature of which cannot be determined by the symptoms alone. Numerous cases of leucorrhœa in young unmarried females, where rational constitutional and local treatment is adopted, perfectly recover, where no examination is made.

In cases of obstinate leucorrhœa, I have often employed the speculum in married women after I had failed to detect the existence of disease by the ordinary mode of examination. In some of these cases, there has been seen an unusual degree of redness of the os uteri, sometimes affecting the whole, and at other times limited to the inner margin, with or without swelling. The white viscid discharge has been seen issuing from the os uteri. I have never seen ulceration of the orifice of the uterus in such a case, and the condition of the interior of the cervix I have never been able to demonstrate either with the bivalve or any other speculum; nor do I believe that, in the ordinary condition of the os uteri, it is possible to see the inner surface of the cervix to any great extent by any means. Where the orifice is unusually open, the lips may be separated sometimes to a small extent; but never, as far as my observation goes, to show more than an extremely small part of the interior of the cervix. In some cases of chronic leucorrhœa, with redness and swelling of the os uteri, I have known the speculum and caustic employed at short intervals for many months without the slightest benefit, but the leucorrhœa has ceased as the general health has been restored by constitutional and topical treatment. In a case of sterility with obstinate leucorrhœa, which has very recently occurred, the injection into the cavity of the uterus of a weak solution of sulphate of zinc, caused the most sudden and excruciating pain, and collapse of the nervous system, which had nearly proved fatal.

Sometimes one or both lips of the os uteri are in the condition which is usually called hypertrophy, and which has no relation to cancer. One lip perfectly smooth, and not unusually hard or irregular, as in cancer, protrudes beyond the other to the extent of half an inch, or three quarters, or more. I have known this state mistaken for polypus, seized with the forceps, dragged down to the orifice of the vagina, and removed with the knife or scissors. At other times both the lips are swollen, nodulated and fissured, and the mucous membrane covering them intensely red, with an appearance of superficial excoriations or granulations, which are elevated above the surrounding surface. These apparent granulations are usually considered and treated as ulcers of the os and cervix uteri, but they do not present the appearances which ulcers present on the surface of the body, or in the mucous membranes lining the viscera, and they are not identical with the granulations which fill up healthy ulcers. They present the appearances often observed on the tonsils, and which are said to be ulcers, but which are not. This granular state of the os uteri, in which the diseased mucous membrane is raised above the level of the surrounding surface, and not depressed like ulcerations in all other parts of the body, is not dissimilar to granular disease of the mucous membrane of the eyelids, the most aggravated cases of which are well known to be produced by the abuse of escharotic applications. These morbid states of the os uteri most frequently indicate the existence of some disease of the nabothean glands, penniform rugæ, lining membrane or walls of the uterus, or of the general health, which lies far beyond the reach of the speculum and caustic. The state of the orifice of the urethra not unfrequently indicates the existence of disease of the prostate gland, or of the urethra itself near the bladder. Such is the case with the os uteri, and its red swollen hypertrophied, granular state, often indicates morbid conditions of the constitution, of the glands, mucous membrane, and walls of the uterus, on the nature, diagnosis, and treatment of which little or no information is derived from the use of the speculum. In these cases, I have known leeches, scarifications, caustic, and the speculum employed upon a great scale, and sometimes I admit (if the reports of patients are always to be trusted to) with apparent temporary relief. Gently rubbing the os uteri with lunar caustic through the speculum, a few times at long intervals has appeared to effect all the good which such local treatment can accomplish. It is impossible that any disease of the os uteri, or any other part of the body, can require twice or thrice a week for six or nine months, the alternate applications of leeches and caustic through the speculum, in the manner which has recently been recommended and practiced; and it is my conviction that rational constitutional treatment and injections, sedative and astringent, will in these morbid conditions of the os uteri succeed ultimately in producing more successful results than escharotics.

In some of these cases, instead of adopting the course which I have now recommended, for the removal of these diseased states of the uterus, potassa fusa has been applied to the os and cervix uteri, a piece of potassa fusa has been run into the cervix and twisted about in all directions, which has produced sloughing and complete disorganization of the parts. In one case, which I saw in a young married lady, this had been done repeatedly, and the patient had nearly perished from peritonitis and the sloughing which followed. I saw this lady some months after, with her general health deeply injured, the lips of the os uteri partially gone, and the parts cicatrized and contracted. I sought in vain for an explanation of the grounds of such practice, and recommended greater caution in the use of potassa fusa; but the advice was thrown away. Twice since, the same experiment has been repeated by the same individual, and with the same results. One of the most learned fellows of this society has communicated to me the history of a case which came under his observation, in which sloughing followed the application of potassa fusa to the os uteri. The cervix uteri presented the appearance of a hard, pale, shining cicatrix. A narrow chink only was to be seen, into which a small bougie could not enter. The orifice was greatly contracted, if not absolutely closed. The eminent pathologist who has communicated to me the history of this case has justly observed, "That if potassa fusa be introduced into the os and cervix uteri, and turned firmly round, or be applied severely, as has lately been recommended, there must be a partial death and destruction of the part, and a state of actual mortification or gangrene induced; and this injury nature can only repair by sloughing, ulceration, cicatrization, and contraction, if not a complete closure of the cervix uteri."

An English physician, eminent in science, after visiting an hospital in Paris, a few days ago, wrote as follows, to a friend in London:—"I have seen some very wonderful things which I will recount to you; a large speculum being passed up to the neck of the uterus, red hot irons are inserted into the neck, right into the os, which is also cauterized on its surface, and as soon as it is done the creatures get up and walk away, and never seem to feel it at all. To-day M. —— found an os wider open than it should be, and so to satisfy his curiosity, he poked an immense long pair of forceps almost three inches into the cavity of the uterus: yet more, some days ago, I saw him pass his stick of lunar caustic into the os, a little way into the neck, then break it off and leave it there." If such practices as these, here described, were only employed in Paris, from whence all the pretended recent improvements in uterine pathology and therapeutics have issued, I should not now have considered it necessary to denounce them to the Royal Medical and Chirurgical Society.—*Med. Chirurg. Transactions*, *Vol.* xxxiii., *p.* 260.

121.—*On the Supposed Frequency of Ulceration of the Os and Cervix Uteri.*—[In a letter to the editor of 'The Lancet,' Dr. Tyler Smith says, with regard to a report in which Dr. Locock is unable to express his belief, "that ulceration of the neck of the uterus must be a very frequent disease:"]

Dr. Locock has assured me he did not make any such assertion in his speech on the above occasion, but that what he really did say, was, that the state of the os and cervix uteri, described by Sir Charles Clark, *and to which the term ulceration has been erroneously applied,* was common enough. It is well known, that all the best writers on the diseases of women in this country, since the publication of Sir Charles Clark's work, have recognised the form of leucorrhœa depending on inflammation of the cervix.—*Lancet, June* 15, 1850, *p.* 718.

122.—ON THE ORIGIN, CAUSE, AND NATURE OF CHRONIC OVARIAN TUMOURS.

By Dr. E. J. Tilt.

Cellular tissue, which resembles serous membranes in its functions as well as in its structure, which separates the organs one from another, and binds them together in one harmonious whole, has, in each of its multitudinous cells, the power of secreting and absorbing the serum by which they are filled. If, therefore, a ball or any foreign substance be forced into the body, the cellular tissue of the part where it is deposited forms around it an adventitious serous cavity, secreting a serous fluid; and thus the foreign body may remain for years isolated, and therefore harmless. In a similar manner, if, from any given cause (and there are many), blood be forced from those channels wherein it was destined to run its continuous course into the substance of the brain, lungs, or any other of our tissues, that ubiquitous cellular tissue receives it, and from itself forms a serous membrane to surround it on all sides; and as the substance thus enclosed is homologous to the inclosing body, it is sometimes completely absorbed, and the accidental cavity diminishes and disappears. Sometimes, however, the cavity remains, secreting and absorbing like any natural serous cavity; and when found by the anatomist it no longer reveals to him the origin and cause of its existence—the blood clot. In other cases this blood clot is only partially absorbed, and thus reveals the origin of the cyst, by its colour and appearance. We may therefore infer that wherever cellular tissue exists, there an hematic cyst may be developed. From the uncontested properties of the cellular tissue, we may also derive the origin of many other accidental cysts, so frequently met with; for every individual areola of this tissue secretes and absorbs; and, as it holds communion with the neighbouring cells, we find them all alike distended in œdema and anasarca. But certain areolæ are cut off by inflammation, or some other cause; and thus arises a serous cavity, the increase of which will depend on the nature of the organ wherein it is formed, as well as on a variety of other causes: cysts of the eyelids and of the mammæ, the synovial bursæ of the patella, the ganglia, or tumours formed on the sheaths of tendons, may be thus satisfactorily explained.

We have thus declared two origins for accidental cysts, as depending —1. On the nature of the areolar tissue, which permits one or more of its cells to set up on its own account; 2. On an hematic origin. We may also add, that whereas there is a great uniformity in the liquid produce of natural serous cavities, the serum being transparent, light-coloured, and of a low specific gravity, there is also uniformity in the nature of the fluid contents of all accidental serous cavities, the liquid being more complex in its nature, of a more considerable density, and often mixed with fatty substances and cholesterine. Applying to the subject under consideration, these deductions from the laws of general pathology, we shall have at least the origins of ovarian cysts; for cellular tissue enters into the structure of the ovary, and blood may also be extravasated into its tissue: and admitting, with De Graaf, and with Logger, that, besides the Graafian follicles there are minute vesicles in the stroma of the ovary,—a fact lately confirmed by the microscopic researches of Henle,—we may believe that they are sometimes the seat of intra-ovarian-fibro-serous cysts; while the extra-ovarian, which are always fibro-serous, and those formed in the broad ligaments, can only be thus explained. Dupuytren, Chaussier, and others, believed that all ovarian cysts were adventitious; but a slight inquiry into the nature of the ovaries will soon convince us that these are not the only original seats of ovarian cysts, and that we may believe, with Cruveilhier, professor Stoltz, and many other writers, that they often originate in the

Graafian follicles. The ovaries are cellular in their structure; and it was necessary that they should be so, to render them fit receptacles for the human germ.

Every month during the productive period of a woman's life, is marked by the enlargement of a follicle, which may or may not burst. If it burst, the remaining cavity will be filled with blood, which may or may not have been absorbed when a succeeding menstrual effort comes to give an increased energy to any healthy or morbid action in which the ovary may be engaged. We have, therefore, in the cystic structure of the ovary, and in the monthly fertility of its soil, additional reasons for the frequency of ovarian cysts; and the possibility of uncontrolled distension, which, from their situation, they necessarily enjoy, is a reason for their increased hydatids.

Very little is known of the origin and causes of hydatid formation in general, and, in particular, of hydatid ovarian cysts. These bodies are formed in fibro-serous cysts; and although the cause of their production is unknown, they may very likely depend upon same unhealthy condition of the blood: but still we know not in what that condition may consist. Thus it has been observed that, in almost every instance of hydatids, the general health has been for a long time previous considerably deranged; and it appears that Dr. Jenner produced hydatids and flukeworms in rabbits, by feeding them exclusively on succulent food. These experiments acquire additional importance since the publication of Dr. Schleisner's account of the diseases peculiar to Iceland: its corn, esculent roots, and fruit, will not grow in that island; the inhabitants eat little bread, or vegetables, and live upon dried fish, dried meat, butter, tallow, train oil and blubber. The probable result of this exclusively animal diet is a frequency of hydatid growths unknown in any other country. Hydatids formed one-eighth of the cases enumerated, and one-sixth part of those observed by Dr. Schleisner, who remarks that in one parish he met with two or three sufferers from this disease in every family.—(Review of Dr. Schleisner's book, 'Brit. and For. Med. Review,' Jan. 1850.)

Unassisted by an imposing array of facts to account for the origin of these productions, we must have recourse to analogy, and to what we have learned from comparative physiology respecting the generation of our tissues. In explanation of these laws, can we do better than borrow from an illustrious countryman, who has already raised himself a name which must descend to posterity, Professor Owen, who, in his valuable lectures on generation, lately delivered at the Royal College of Surgeons, says,—"The primitive forms of all tissues are free cells, which grow by imbibition, and which develope their like from their nucleus of hyaline. All the animal tissues result from transformations of these cells. It is to such cells that the acephalocyst bears the closest analogies in physical, chemical, and vital properties. When the infusorial monads are compared to such cells, and man's frame is said, by a figure of speech, to be made up of such monads, the analogy is overstrained, because no mere organic cell has its mouth, its stomach, its pulsatile sac, &c. So also it appears to me that the analogy has been equally overstrained, which makes the acephalocyst a kind of monad, or analogous species of animal. We may, with some truth, say that the human body is primarily composed, or built up of acephalocysts: microscopical, indeed, and which, under natural and healthy conditions, are metamorphosed into cartilage, bone, nerve, muscular fibre, &c. When, instead of such change, the organic cells grow to dimensions which make them recognizable to the naked eye, such development of acephalocysts, as they are then called, is commonly connected in the human subject with an enfeeblement of the controlling plastic force, which, at some of the weaker points of the frame, seems unable to direct the metamorphosis of the primitive cells along the right road to the tissues they were destined to form, but permits them to retain, as it were, their embryo condition; and to grow by the imbibition of the surrounding fluid, and thus become the means of injuriously affecting or destroying the tissues which they should have supported and repaired. I regard the different *acephalocysts*, therefore, as merely so many forms or species of morbid or dropsical cells.

"The question which remains to be solved is, how the parent hydatid originates; two modes may be conjectured: either it is the result of the development of an organic granule, nucleus, or cell, of the animal invested, according to the hypothesis of fortuitous or spontaneous generation; or it is developed from an impregnated germ-cell detached from a pre-existing acephalocyst, and potentially endowed with the special mode and direction of the plastic force which issues in the repetition of the same organic form as the animal from which it proceeded. Analogy points to the latter as most accordant with actual physiological experience."

Whatever may be the cause of these singular bodies, when once originated they may for years continue to re-produce similar bodies. The late Dr. Ryan has recorded a case in which hydatids were retained in utero so long as fourteen years.

Piliferous cysts.—In some rare instances the impregnated germ falls not into the fallopian infundibulum, but remaining attached to the ovarian birth cell, gradually appropriates to itself the tissue of the ovary, and so distends it as often to attain a considerable size. These fœtal cysts may remain stationary for years. Sometimes the fœtus is reduced to a kind of mummy, but at others it undergoes decomposition, the soft parts being converted into various kinds of fatty matter. While portions of skin of the fœtus become attached to the interior of the cyst, redroducing by its bulbs the hair which is so often found in considerable quantities, the more solid parts, such as the bones and teeth, remain to indicate the origin of the cyst.

We are thus led to infer, that piliferous cysts are sometimes only fœtal cysts, wherein the greater part of the body has undergone decay. But does this explanation hold good in all cases of piliferous cysts? M. Pignè, who has paid great attention to this subject, does not hesitate to assert that all piliferous cysts are either the result of extra-uterine pregnancy, or of a malformation; that, for instance, when piliferous cysts are found in girls of a tender age, or in young boys, we must believe them to be relics of some other individual contemporary with one in whose body they are implanted; and that it was in an early stage of their existence that somehow or other one of the two became included in the other. Those who must have an explanation of the fact, and who can never, as Montaigne so quaintly says, "make up their minds to lay quietly their heads on the pillow of doubt," may admit this explanation, but it is surely better to confess our ignorance than attempt to explain the inexplicable. If we had merely to account for those piliferous cysts growing in the vicinity of the shin, in the scalp, the eyelid, the mamma, and the scrotum, we might, as Carswell suggests, consider the hair they contain as arising from accidental development, as it grows from the anterior portion of these cysts which is lined by the reflected cutaneous tissue; but such an explanation could not account for the hair and teeth found in the ovaries of virgins, in the coats of the stomach and of the bladder of man, in bronchocele as mentioned by Celsus, in the abdomen of a boy by Dupuytren and Mr. G. W. Young, in the lungs by Möhr, in the brain by Morgagni, and in the orbit by Barnes. (Med. Chir. Trans. vol. iv.)

Without pretending to explain the fact, we must observe that in all well-authenticated cases the interior of the cyst has been found to be lined with a substance like the skin, with bulbs producing hair. Cruveilhier, Velpeau, Carswell, and many others, have seen cysts partially lined with skin; in a case recorded by Bricheteau, the cyst was entirely so lined, and this must also have been the case in the instance quoted by Morgagni, from the author of an opuscule, *(de Ovarii tumore pilosâ, Liepsick*, 1735), who does not scruple to compare the cavity of the cyst to the scalp. It must be a tissue similar to skin, for it not only secretes fat and sebaceous matter, but hair has been repeatedly seen to grow from its bulbs, horny productions have been known to spring from it, dental sacs and teeth have been found rooted in it: and these are all epidermoid productions. How a fragment of skin could thus be found we cannot explain, but it seems as if the vegetative life adherent to each of the germ cells which constitute our frame, were able sometimes to produce the "horn layer," (mucous layer), or that portion of the germinal layer which covers the embryo, and is the foundation of the non-vascular and nervous tegumentary appendages. Here, again, we willingly quote from Professor Owen's second lecture on the generation and development of invertebrated animals, in order to bring the light of comparative physiology to bear on the more abstruse points of pathology.

"In many classes of the invertebrate animals the retained spermatic force operates with such energy as to form a new individual from a secondary, tertiary, or quaternary derivative germ mass. How this is done we know not; suffice it to say that it is done. The completion of an embryonic or lowest form by the development of an ovarian germ-cell, as in the aphis, without the immediate reception of fresh spermatic force, has never been known to occur in any vertebrate animal; in the vertebrate animal, the whole of the spermatic force, originally diffused amongst the cells or nuclei of the germ mass, is exhausted in the development of the tissues and organs of the individual, in the mysterious renovation of the spermatic power in the male, by a special organ; and in the development of ova or cells prepared for its reception in the female. But it now and then happens, even in the highest of the vertebrata—the human species—that the ovarian cells sets up the process of embryonic development, but without sufficient of the spermatic and plerotic power to complete even a lowest form: some crude materials of the embryo are the only result,—teeth, it may be, or hair, with irregular amorphous ossifications, such as are met with occasionally in ovarian cysts."

Whatever may be the origin of such epidermoid appendages, they are more frequently found in the ovary than in any other organ, and Meckel asserts that they are more frequently found on the right side. They are met with at all ages: in 49 cases collected by M. Pigné, 5 were not 12 years old, the age of 6 varied from 6 months to 2 years, 4 were full-grown fœtuses, and piliferous cysts were also found in the ovaries of 2 fœtuses which had been cast off before the end of the ninth month.

These cysts may communicate with the bladder, as in the case given by Suetin, (An. de Med. Belge, 1838), where a tooth was found forming the nucleus of a stone in the bladder. Hair has likewise been passed in the urine, by a communication between the bladder and a piliferous cyst, but Magendie has published cases of gravel, wherein hairs were passed mingled with the usual saline conditions. Where did these hairs come from? Were they secreted by the kidneys?

It is well to know that such productions are not peculiar to the human species, and that the hair of the cow and the wool of the sheep have been found in the ovaries of these animals.

Penada found a fatty bag containing two bunches of feathers, 33 in number, in the breast of a young cock.

[Considering the exciting causes of ovarian tumours, Dr. Tilt says,]

Before attempting to show how the causes of ovaritis produce chronic ovarian disease, it is necessary to observe that most morbid anatomists admit the inflammatory origin of adventitious cysts. Without burdening ourselves with numerous citations, we shall quote the opinion of one of our most eminent authorities on such matters. "Although the origin of adventitious serous cysts is in a great many cases involved in much obscurity, there are good reasons for believing that it is similar in kind to that of adventitious serous membranes; that is to say, that these cysts originate under the influence of causes which excite inflammation of the cellular tissue in circumscribed portions of organs" (Carswell's Pathological Anatomy). We only affirm, then, that cysts are formed in the ovary by a process similar to that which produces them in other parts of the body; and we also add that, as under healthy circumstances, the ovary devotes its whole energies to bring the menstrual cysts to perfection, so under the influence of inflammatory causes it devotes the same energies to produce encysted growths of a morbid nature. In no system is inflammation more susceptible of being limited to a *very* small space than in the sexual system, for the neck of the womb may be severely inflamed without the body in the least participating in it. Huguier affirms that the follicles of the uterine mucous membrane (the ovula of Naboth ?) are sometimes found dilated and inflamed, while the other portions of the mucous membrane do not participate in the inflammation. Cruveilhier and other observers have sometimes seen one uterine vessel full of pus, offering distensions about the size and shape of small olives: the fallopian tubes may also be independently inflamed; one or both ovaries may be so; a single ovary may be inflamed in its stroma, in its peritoneal coat, in certain portions of its tissue, or in one menstrual cell which may alone be found containing pus or well organized false membranes. When we talk of partial ovaritis producing the different forms of chronic ovarian disease, we merely affirm that inflammation developes, in a morbid degree, the structural tendencies of our organs. Inflammation of the bony structure will cause a greater deposition of saline particles in their cellular texture, that of the serous membrane an increase of the usual fluids and other products; in like manner partial ovaritis developes the peculiar cystic structure of the ovaria by exaggerating the prolific tendency of its elementary tissues. When once the cyst is originated, and the balance between secretion and absorption on its internal surface is interrupted to the advantage of the secreting powers, there is no reason why exhalation should not continue predominant in artificial encysted cavities, as well as in those cavities which are natural. We believe inflammation to be as much the key-stone of ovarian disease as it has been proved to be of uterine pathology, and of the structural lesions of other organs. We believe that it lends a hand in the formation of almost every ovarian cyst, even when that inflammation is too circumscribed to be detected by any means we have at our command; and we thus admit that it is alone a cause sufficient for the very numerous cases of fibro-serous variety, and of ovarian growths, even when they are more or less solidified into cartilage, fibrous substance, or bone. If, however, the blood be loaded with constitutional impurities with the occult seedlings of cancer, or the original elements of that protean diathesis called scrofula, the existence of which is clearly established, however difficult it may be to define it characteristically, then, if the ovaria are under the influence of any of the causes of ovaritis, they will withdraw from the blood which flows continually towards them the elements of disease, and will so assimilate them as to build up cancerous (colloid) encysted ovaria. The warp and woof of the cancerous tissue is afforded by the primitive structure of the organ. The cancerous diathesis finds the rough materials which are to be employed, and inflammation is the all-powerful agent which works up this rough material into the tissue of the organ. Thus we account for the production of areolar cancerous ovarian cysts.

When the follicular multicystic forms are found to exist in patients presenting signs of scrofulous tendency, we can then in a similar way account for the strange and different productions we have described; but in many cases we cannot explain them by any constitutional affection. We must, however, bear in mind that it is but fair that the organ which has within it sufficient energy to secrete the living man should also have the power of giving spontaneous growth to productions of an inferior nature; and that in the whole history of the ovary there is an uniform striving for production, which is equally shown by its physiology and its pathology; and thus, if the organs of increase are useless for fruitful progeny, they will often teem with spurious growths. The streams of living productivity having been once made to rise in those hidden wells of our existence, they cannot be dried up. They will often go on producing imperfect growths or forms which, being perfect, can reproduce themselves; and so it is throughout the whole creation.—*Med. Gazette, Aug.* 9, *p.* 234, *and Oct.* 18, 1850, *p.* 660.

123.—ON THE DIAGNOSIS OF OVARIAN DROPSY.

By I. B. Brown, Esq.

[Briefly alluding to the diseases simulating, and which might be mistaken for, ovarian dropsy, we have, says Mr. Brown,]

1. Retroflexion and retroversion of the uterus.
2. Tumours of the uterus.
3. Ascites.
4. Pregnancy.
5. Pregnancy complicated with ovarian dropsy.
6. Cystic tumours of the abdomen.
7. Distended bladder.
8. Accumulation of air in the intestines, especially if there has been chronic peritonitis leaving some ascitic fluid.
9. Enlargement of the solid viscera of the abdomen, the liver, spleen, and kidney.
10. Accumulation of fæces in the intestines.

Respecting the first, ovarian dropsy, when it is first detected in the pelvic cavity, between the rectum and vagina, may be confounded with retroversion of the uterus, but a careful examination of that organ will decide the point, for, in retroversion, the os uteri is thrown forcibly forwards and upwards, and the womb is immovable and painful; not so in ovarian dropsy. Retroflexion of the uterus, which has been well described by Dr. Rigby, more closely resembles ovarian dropsy, but on examination by the uterine sound, and by careful manipulation, the fundus of the uterus can be placed in its natural position.

2. Tumours of the uterus, particularly those with distinct peduncles, may at first be mistaken for ovarian dropsy, but a careful examination, first of the uterus itself, and then of the tumour, with the absence of fluctuation and elasticity, will soon determine the point.

3. Ascites may be, and is, more generally mistaken for this disease. But in ascites the general signs are, swelling of the extremities, and consequent inability to walk; disturbance to digestion, inadequate action of the liver: respiration, and the action of the heart, are all sensibly affected, both because the thoracic organs are themselves sometimes the seat of disease, and also because those organs are compressed upwards by the unusual elevation of the diaphragm. This disease is usually ushered in by irregular or suppressed action of the liver or kidneys, or by chronic peritonitis; the effusion of fluid is general over the abdominal cavity. On placing the patient on one side, the fluid depends to that side, and the sound of the bowels, on percussion, can be heard over the upper side. At the earlier stages, the effusion is seated at the depending portion of the abdomen; on applying pressure firmly over this depending portion, the fluid will be dispersed, and on percussion the sound of the bowels can be heard. The shape of the abdomen is also altered by any position, as upright or horizontal; the fluid, in fact, occupies the cavity of the abdomen from ensiform cartilage to pubes—that is, the upper, middle, and lower thirds of the abdominal cavity. On placing the patient in the upright posture, the fluid descends so as to render the presence of the intestines perceptible in the upper third of the abdomen.

4. Pregnancy is sometimes mistaken for ovarian dropsy, and in the first two or three months of utero-gestation, its discrimination often requires great tact and careful examination, for in ovarian dropsy you sometimes have enlarged mammæ, with dark and extended areolæ around the nipple, and also moisture from the nipple itself, with suppression of the menstrual discharge and morning sickness. A case lately came under my notice, where the patient had been pronounced, by more than one medical man, pregnant; but as constitutional derangement increased, and she did not increase in size so fast as was usual in pregnancy, she consulted me, and believing that it was a case of ovarian disease, with that peculiar thick, cheesy matter already referred to, I examined the uterus by the sound, and ascertained that no pregnancy existed. Subsequent tapping proved the correctness of the diagnosis. I was also called suddenly to another lady, who had been most carefully examined by a distinguished obstetric physician, (not generally mistaken in his diagnosis,) who declared that she suffered from ovarian dropsy, and yet, on my arrival, I found her in premature labour of five months' pregnancy, induced probably by the examination she had undergone.

Pregnancy complicated with Ovarian Dropsy.—Now this is perhaps the most difficult of all to distinguish and determine. I have met with three cases of this rare complication. In one I was called to see the lady pregnant with her second child. I found her generally ill and weak, complaining of the enormous size of her abdomen, and satisfied in her own mind that she should have twins. At the proper period labour came on, and the child was born without difficulty; but on placing my hand externally, to grasp the uterus, I found I could not feel it, for the pelvic cavity was filled by a soft, elastic tumour, and the uterus had ascended out of the pelvic cavity, and was above this tumour. On introducing my hand into the vagina, and endeavouring to reach the uterus, to remove the placenta, and pressing my other hand externally over the uterus, I had the pleasure to feel the tumour, which I felt was an ovarian cyst, suddenly rupture, and discharge its clear, amber-coloured fluid down the side of my arm. The uterus now descended, the placenta was removed, and a very tight bandage applied, and kept on for several weeks; and on a subsequent confinement, not a vestige of this tumour could be felt. In the second case, I pronounced my opinion to be, that there was ovarian dropsy, independently of pregnancy. This patient was safely delivered of a full-grown child, and subsequently I tapped the cyst, removed sixteen pints of fluid, and applied tight bandaging. This patient suckled her infant for twelve months. In the third case, the patient was delivered in the country, and came to me directly after her confinement. Tapping and pressure were resorted to successfully.

5th. Cystic tumours of the abdomen are by far the most difficult to discriminate from ovarian dropsy, and it is only from the earliest history and position that we can draw any probable conclusion; and in the enlarged forms of these tumours it is extremely difficult to diagnose accurately.

6th. Distended bladder may be, and has been, mistaken for ovarian dropsy. A case of this kind came under my notice last year. A young lady, aged twenty-three, came up from the country, stating that she had been under treatment for four months, for "falling down of the uterus," but that during the last month she had become very much enlarged in the body, and her medical attendant thought she was suffering from ovarian dropsy, and therefore she consulted me. I found, on examination, a round smooth tumour, the size of a fœtal head, rising up from the pubic region, with distinct fluctuation. She stated that she had passed but very little urine for some weeks, and then only in very small quantities at a time. On examination per vaginam, I found a retroverted uterus, the os and cervix pressing firmly up against the neck of the bladder. On replacing the uterus by the uterine sound, and pressing on the tumour through the abdominal walls, urine escaped through the urethra; I then introduced a catheter, and drew off seven pints of dark, offensive urine, and the tumour disappeared never to return.

7th. Accumulation of air in the intestines, especially if there has been chronic peritonitis, leaving some ascitic fluid. I shall presently illustrate this more fully by relating a case in detail.

8th. Enlargement of the viscera of the abdomen, especially of the liver, of the spleen, or of the kidney. I could illustrate this subject by mentioning some curious cases of error in diagnosis, in connexion with each of those organs, but I shall merely mention, that in these cases we generally have severe constitutional symptoms pointing out the nature of the disease.

Mr. Harvey related a case of great interest last session at the London Medical Society. It was supposed to be a case of ovarian dropsy; ovariotomy was determined on, but not executed, and when the patient died, the disease was found to be an hydated cyst, connected with the liver, no ovarian disease whatever existing.

9th. Accumulation of fæces in the intestines is another form of disease not unfrequently mistaken for ovarian dropsy. I once met with a case of simple encysted ovarian dropsy, which, at the earliest growth, was considered by a very distinguished surgeon in London to be an accumulation of fæces, and yet the subsequent history of the disease, terminating in tapping, and followed by pressure, not only proved the true character of the disease, but also resulted in a permanent cure.—*Lancet, July* 13, 1850, *p.* 47.

124.—*New Operation for Ovarian Dropsy.* By I. B. Brown, Esq.—[Mr. Brown, in bringing this operation before the Westminster Society, stated the difference from that of Mr. Bainbridge, as it enabled the patient to lie on her back on water cushions, allowed the wound to be dressed and cleaned readily and without fatigue, and also enabled the surgeon to make pressure on the cyst by strips of plaster carried round the abdomen. He placed the patient on the edge of the bed in the horizontal position, and then drawing an imaginary line from the umbilicus to the superior spinal process of the ilium, and dividing it into thirds, he made an oblique incision from above downwards, and from within outwards, through part of the middle third and into the outer third.]

The oblique incision was about three inches. He (Mr. Brown) dissected carefully through the muscles and fascia down to the peritoneum; through this could be seen the whitish shining coat of the cyst; he then made another smaller incision at right angles to the first, and dissected down to the peritoneum; this incision was about an inch and a-half in length. At the point of the angle a large-sized trocar was introduced, and the fluid drawn off. The canula being kept in, the peritoneum was divided, and reflected back; then the cyst was stitched by sutures to the aponeurotic tendon of the external abdominal oblique muscle, carefully avoiding the other structures; these sutures were nine in number, and were so arranged as to secure completely the cyst on all sides to the tendon of the muscle, and prevent any escape of the fluid from the cyst into the peritoneal cavity; the canula was now withdrawn, and by means of a pair of scissors the cyst was divided between the sutures; a pledget of lint, soaked in oil, was then introduced into the wound, and changed occasionally, and some adhesive straps placed across the abdomen, to keep up gentle pressure. Not a single bad symptom, referable to the operation, followed. The second operation was performed in a similar manner, and in the same position, but a piece of the cyst was cut out, the fluid being first evacuated, and then the remaining portion of the cyst was allowed to return to the peritoneal cavity, the external wound being closed; pressure was applied over the whole abdomen by means of adhesive strapping and flannel bandage. This operation was intended to imitate spontaneous rupture, and was only to be recommended where the fluid was not of an irritating character.—*Prov. Jour.—Monthly Journal of Med. Science, Aug.*, 1850, *p.* 179.

125.—ON DISPLACEMENT OF THE OVARY.

By Dr. Edward Rigby.

During the last three years (says Dr. Rigby) I have had occasion to notice several instances of a painful affection of the pelvic region, which I had not observed before, and which as far as I know, has not yet been described. It is characterised by intense and peculiarly sickening pain about the sacral region, extending to one or other of the groins, and coming on in paroxysms of such agonising severity as to render the patient perfectly frantic with the intolerable suffering. In some patients the intermissions of ease were nearly or quite entire; in others, the pain, although divested of its characteristic intensity, never wholly abated. The source of the pain was evidently connected, directly or indirectly, with the rectum, for the passage of fæces was frequently attended with some difficulty, and always with great suffering.

According to the patient's feelings, it seemed as if a partial obstruction existed somewhere up the rectum, the smallest pressure upon which, by the passage of fæces, was sufficient to bring on a paroxysm of this much dreaded pain. At other times, she could scarcely tell what had been the exciting cause of the attack; for, like a fit of tic douloureux, it would frequently come on from no assignable reason, and cause her the severest sufferings for some hours. Patients describe the pain as being quite peculiar, and of a sickening and utterly intolerable character, such as they had never before experienced; indeed, from the descriptions which one or two have given of it, I should presume that it closely resembles the intense and peculiar suffering which patients describe in cases of orchitis. The pain was usually attended with great throbbing, and with a painful sense of forcing, or distension of the tender part, amounting almost to bursting, like something strangulated.

The menstrual periods have always been attended with intense pain, particularly during the early part of the discharge, though this varied a good deal in different patients, and (as far I have seen) the discharge was invariably attended with exudations and small coagula. At these times the whole lower part of the abdomen was frequently tender to the touch, and more or less fever was generally present, probably arising in part from the degree of suffering which had been induced. The tongue always showed the dry short-napped fur which is so constantly seen in cases of disease, or displacement of the pelvic viscera; the digestive organs were much deranged, and not unusually the stomach was extremely irritable.

On making a vaginal examination, she would frequently wince, and complain as soon as the finger touched the os and cervix uteri; but a little care quickly sufficed to show that these parts were not morbidly tender, but that pain was produced by pressing them against the tender spot which was behind and to one side, in the direction of one or other of the sacro-iliac synchondroses, or sacro-ischiatic notches. On passing the finger, therefore, behind and to one side of the cervix, and pressing against the wall of the vagina in the above-mentioned direction, the painful spot is at once reached, and sometimes a slight degree of hardness is perceived.

On examining per rectum, the finger soon reaches the same acutely painful spot which had been felt per vaginam. The patient dreads the slightest touch of it, however carefully applied. It is evidently a convex body, like an enlarged gland, though usually softer, situated in the recto-vaginal pouch; it is moveable, if the patient can bear a sufficient amount of pressure for that purpose, and usually one or more vessels are felt throbbing when the finger presses upon it. The ovary is generally larger than natural, being more or less swollen from the strangulation produced by its displacement; and when the swelling is considerable, not only will pain be produced by pressing in the groin of the same side, but the ovary will be distinctly moved upon the finger per rectum. From the fact of its mobility can be explained the circumstance of our being able to feel it sometimes lower in the pelvis than at others, and why the patient is in greater suffering when it is so, than when felt higher up. Hence the passage of a solid mass of fæculent matter is attended with fearful sufferings; the ovary is pushed down by the fæcal mass as it descends through the rectum, until its attachments are put considerably on the stretch; a further amount of swelling is produced by this state of strangulation, and, in this condition, the fæcal mass is at length forced past, to the indescribable agony of the patient, frequently leaving her in severe pain for many hours afterwards.

In other cases, the ovary is nearly or quite fixed, having apparently contracted adhesions to the neighbouring parts.

It is not easy to speak decidedly as to the cause of this displacement, but I have chiefly, or almost solely, observed it among women of a lax, flabby habit, prone to passive menorrhagia, leucorrhœa, and abortion, but most particularly where the uterus has been retroverted. I have long since pointed out, that ovarian irritation or inflammation is a frequent result of retroversion in the unimpregnated state, arising probably from the tension to which the broad ligaments are exposed, and consequent engorgement of the ovary.

The diagnosis is not difficult, for the pain is quite peculiar. It is of a forcing, throbbing character, so sickening and utterly intolerable, as to be entirely different to any other pelvic pain with which I am acquainted. Its seat is referred to the upper and posterior part of the vagina, usually somewhat to the left side where the ovary can be felt, especially upon examination per rectum.

The direction in which we can best reach the painful spot will guide us as to applying leeches per rectum or vaginam; they pretty certainly produce much relief, although, so long as the displacement continues, it cannot be more than temporary. The grand object necessarily must be to restore the ovary to its natural position, and this, whether connected or not with retroversion, will be best obtained by the prone position. The patient soon learns by experience what amount of lying forwards will be required, but, as in cases of retroversion, it is generally found necessary to assume an extra degree of prone position for a few minutes (knees and elbows) previous to lying down upon the prone couch.

On making an examination per vaginam, one might have been induced to suppose that the cervix uteri was the part affected, from the intense pain which was produced on touching it; but a little attention sufficed to show, that the cervix itself was not the source of her pain, but had only produced pain when pressed against the painful ovary. The ovary itself was felt most distinctly both per vaginam as well as per rectum. Its position varied a good deal at different times; when in most suffering it was low in the recto-vaginal pouch, nearly corresponding to where the fundus uteri is felt in a case of retroversion. After using the prone position it receded considerably, and her sufferings diminished in proportion. The case was one in which I could do but little while she was an out-patient beyond attending to the general health. I therefore admitted her into the hospital for the purpose of having leeches applied, and for the sake of ensuring that entire amount of rest in the prone posture which her symptoms required. This position always relieved her, but a decided impression was not made upon the swollen throbbing ovary until the application of leeches.—*Med. Times, July* 6. 1850, *p.* 6.

126.—*Case of Fibro-Cystic Tumour of the Uterus, which required Tapping; with Remarks.* By PRESCOTT HEWITT, Esq., Assistant-Surgeon to St. George's Hospital.

M. G., æt. 47, single, was admitted into St. George's Hospital, under the care of Dr. Wilson, with great swelling and distension of the abdomen. The symptoms, which had existed about twelve months; had been at first confined to the left iliac fossa, but had subsequently spread over the greater part of the belly. Fluctuation was very evident in various regions, and the disease presented all the characters of ovarian dropsy. Œdema of the legs was present, as well as pain in the region of the heart, and difficulty of breathing in going up stairs. The general health had not been much affected, but of late she had lost flesh; the catamenia had been absent for the last six months; the urine was scanty and highly acid. She was put on diuretics and good diet. After five days it was found that she had decreased two inches in circumference round the abdomen, and that there was much less swelling of the feet. Under this plan of treatment she at first continued to improve slightly; but the symptoms and consequent distress having subsequently increased, Mr. Hawkins tapped the abdomen, and drew off fifteen pints of thick fluid of a reddish colour, and mixed, towards the last, with blood and some flakes of lymph. After the operation, it was observed that the decrease in size had occurred principally on the left side, and two masses of solid substance were detected, which appeared to form part of a tumour, rising from the pelvis. The operation was, at first, followed by marked relief; but, two days afterwards, symptoms of low peritonitis appeared, and the patient died, on the eighth day after having been tapped.

Examination of the body, eighteen hours after death.—The cavity of the peritoneum contained a large quantity of dark-coloured fluid, mixed with flakes of recently effused lymph, which served to glue together the convolutions of the intestines. In its lower two thirds, the abdomen was occupied by a large tumour, which, rising out of the pelvis, had displaced the intestines, and become attached, by slight adhesions, to the anterior wall of the belly. The upper part of this tumour was composed of large membranous-looking cysts, with thin walls, the interior of which was inflamed, and filled with a quantity of thick, dark-coloured fluid. It was one of these cysts which had been tapped during life. Towards its lower part, the tumour was principally formed of a more solid substance, and filled with an enormous number of cysts, varying from the size of a pin's head to that of a large orange. These cysts, which were all lined with a thin, smooth, delicate-looking membrane, were filled with clear fluid, containing a large quantity of albumen. The diseased mass was, at first, thought to be connected with one of the ovaries, but both these organs were found to be lying behind it, and quite healthy. On further inspection, the tumour was traced to the right side of the fundus of the uterus, to which part it was connected by means of a pedicle, two inches in breadth, and an inch and a half in length, formed by the fibres of the uterus, which were traced upwards some distance, and then lost. Among these fibres, were several vessels of a large size. Here and there, in the lower part of the tumour, were scattered some spots of fibrous tissue, hard, dense, and without any cysts. In the body of the uterus, deeply imbedded in its structure, there was a common fibrous tumour, of the size of a bean. There was no affection whatever of any of the glands. The other viscera contained in the abdomen, as well as those in the thorax, were all quite healthy.

Microscopic examination proved that the tumour was essentially of a fibrous character.

Remarks.—Accumulations of fluid, in connection with fibrous tumours of the uterus, may occur under various forms. As they present all the characters of ovarian dropsy very strongly marked, such cases are of great importance, for they have sometimes led to errors in diagnosis, and to useless operations; especially those cases in which the fluid is poured out into numerous small cysts.

The fluid is sometimes contained in a single cavity, formed by the softening and subsequent removal of the centre of the tumour, the place of which becomes filled up by serum. This, by gradually distending and thinning the outer part of the tumour, may make the disease appear as if it had been, from its origin, cyst. The quantity of fluid contained in cavities thus formed may be very great.

In other cases there are other cysts containing fluid, which are produced in a different manner. Here, the cavities lying between the fibres constituting the tumour, are generally towards its circumference, and that principally in cases where the mass, having grown more rapidly than usual, has acquired a large size. In a fibrous tumour of the uterus, weighing fifty-four pounds, which I dissected some time ago, I found several cavities, which were all situated towards its circumference. Some of these were round, others of a semi-lunar shape—the latter appearing to have been the original shape of the greater number of the cysts. Their formation may be thus explained. The tumour was of a pear-like shape, and composed of layers of fibres, presenting a somewhat looped appearance. The extremities of these layers, being firmly connected, formed a perfectly solid mass; whereas the centre of the loops, towards the circumference of the tumour, being but loosely connected by thin cellular tissue, had, in many places, left inter-laminar spaces, which had subsequently become filled with fluid. The cysts thus formed were of various sizes, and few in number; their cavities were perfectly smooth, and not subdivided by bands of cellular tissue.

There is another class of fibrous tumours, which appear to have been formed by the union of several smaller ones by means of loose cellular tissue, in the meshes of which fluid has been thrown out, and has subsequently formed cavities or cysts. In these cases, the cavities, generally speaking, are not of any great size, and the mass looks like a large œdematous fibrous tumour. When punctured, but little fluid escapes from them; but, if incisions be made in several places, the greater part of the fluid will drain off in a few hours, leaving a coarse, cavernous-looking tissue.

The fibro-cystic tumour described in the case of M. G., cannot, I think, be looked upon as belonging to either of the three varieties already mentioned. From its earliest formation, the tumour was most probably of a fibro-cystic nature, and exactly resembled that form of disease so commonly observed in the ovary, which is composed of unilocular cysts, of various sizes, closely aggregated together, with more or less delicate fibro-serous walls; in fact, so close was the resemblance, that for some time the mass was taken for a diseased ovary. Portions of it also closely resembled the simple cystic disease of the testis, as well as one form of fibro-cystic disease which I have met with in a large tumour of the breast. The accumulations of solid fibrous tissue observed in the case of M. G., were scattered, and small in comparison to the size of the tumour; whereas the mass of it was made up of cysts, which existed in every part, even close to the root.—*London Journal of Medicine, July,* 1850, *p.* 611.

127.—SERO-CYSTIC SARCOMA OF THE MAMMARY GLAND.

Reported by HOLMES COOTE, Esq., and revised by W. LAWRENCE, Esq., F.R.S.

[In this case, Mr. Lawrence states, a lady aged 55, of naturally pale complexion and nervous temperament, noticed, 30 years ago, a small tumour in the right breast, the size of a filbert, which remained stationary, however, causing no inconvenience until four years ago, when it slowly increased and became occasionally painful. About the spring of the present year it had attained considerable size, the integument covering it being raised into irregular prominences, and discoloured. Then ulceration ensued, and from the opening protruded a red fungous mass, from which rather free bleeding took place on three or four occasions. Mr. Lawrence removed the tumour, which he describes as follows:—]

The right breast is enormously enlarged, the increased bulk being caused by a diseased state of the mammary gland. About one-half of the swelling at its base is covered by integument, thinned by distension, but not unhealthy nor morbidly adherent. The other half is a vast fungous protrusion, measuring about eight inches in its transverse diameter by five or six in the vertical direction. This is bounded by a raised collar, the thickness of the thumb, in which the integument and the diseased mass are completely blended. The exterior and the convexity of this elevated boundary are covered by a thin and perfectly smooth epithelium. The interior surface and the excavation which it bounds are nearly smooth and raw. Before removal it was a pinkish red, and not very sensitive; it yielded a thin yellow discharge, not offensive, but so abundant as to require change of dressing three or four times in the day. The mass, being pendulous, lay over the lower part of the chest and neighbouring portion of the abdomen, of which the integuments, constantly wetted by the discharge, were bright red and partially excoriated.

The breast, which was not painful when examined, was moveable upon the subjacent parts, and there was no glandular enlargement, nor any trace of disease in the axilla. The health was good; the patient slept well, had a fair pulse, and ate and drank with appetite.

Under these circumstances I had no hesitation in recommending the operation; for, although it might at first appear formidable to undertake the extirpation of so large a growth, yet two incisions embracing the neck of the tumour, which was overhung by the protruding mass, were sufficient for the purpose. There would be no great loss of integument, no depth of wound, nor any exposure of the axilla.

The patient having assented, the operation was performed Oct. 9.

She was placed on a couch in the recumbent posture, and chloroform was administered. The breast having been drawn up and well supported, in consequence of its size and overhanging position, by a folded towel, the under incision was first made and the skin turned back: then followed the upper incision, carried along the body of the swelling, so as to form a flap nearly three inches wide, when the whole mass was readily separated by a few strokes of the knife from the loose and healthy areolar tissue which connected it to the subjacent pectoral muscle. Several arteries bled freely, but were quickly secured, so that there was no considerable loss of blood. The edges of the wound came nearly together, and the patient was put into bed. There was some bleeding in the evening caused by sickness, with vomiting; but it ceased on the application of cold, and she passed an excellent night.

The breast, which weighs 2 ℔. 6 oz., presents at its base a firm lobulated mass. I now cut into it through the middle to show you the internal structure. You see that it has undergone that morbid change called cystic sarcoma. It is a firm lobulated mass, composed of various sized cysts, filled, in the specimen now before you, with a soft, friable, vascular substance of light yellowish grey colour. The cyst-walls having been distended and thinned have given way, and have allowed the protrusion of the red fungous growth which, with everted edges, occupied the front of the patient's breast. The nipple is concealed amongst the folds of integument.

The disease, which is of innocent nature, commences by enlargement of the lactiferous tubes in the substance of the gland. These become tortuous and varicose, and the naturally oily secretion which they contain is converted into a thin watery fluid. I here show you, from the museum of the hospital, the preparation of a breast which I removed some years ago from a foreign lady. It was carefully dissected by Mr. Coote, who has introduced bristles from the nipple into some lactiferous tubes, dilated to the diameter of a writing quill. In course of time portions of the tube become more or less isolated, and the cavity is gradually distended by secretion into a cyst from the walls of which may produced a soft growth, which first filling the cyst, and then, making its way through the containing structures, emerges as a red and bleeding fungus. Upon dividing a breast so degenerated, we observe escaping from the different cysts fluids differing in consistence and colour,—in some parts thick and glutinous;—in others oily,—in others limpid and watery. The fluid may be either clear and transparent, or of various shades, from a light yellow to a deep brown tint, from admixture of blood.

As contrasted with the extirpation of a breast affected with scirrhus, the operation of removing one of these innocent tumours, which are always loosely connected to the neighbouring structures, is comparatively easy. In the present case, the diseased parts were firmly connected only in one spot, where the areolar tissue was thickened, probably from the weight of the mass. In malignant diseases, the infiltration of morbid deposit always extends beyond the structures first affected. Hence it is often necessary to make the incisions at some distance from the obvious circumference of the tumour, and to include a considerable portion of integument, the removal of which prevents the approximation of the edges of the wound, and involves the necessity of our leaving an open surface of considerable extent—a condition which adds to the patient's danger, especially when the wound is situated over a cavity containing such important organs as those of the chest.

[Mr. Lawrence remarks on the administration of chloroform in this case, that although it prevents physical suffering, obviates the severe nervous shock attending great operations, and thus leaves the patient in a much more favourable state for recovery; yet as there are some well-authenticated cases where death has resulted from it quickly, even where the dose has been small, he advises the following cautions to be observed: to administer it slowly, and to allow its dilution by the admission of the atmospheric air—to watch the pulse, and to remove the apparatus when the circulation begins to fail. At the same time he states, as an important fact, that neither in this hospital or private practice has any alarming circumstance occurred hitherto to cause him any serious apprehension.]—*Med. Times, Nov.* 9, 1850, *p.* 477.

128.—*Case of Scirrhous Degeneration of the Mammary Gland.* By W. Lawrence, Esq., F.R.S., &c.—[The patient in this case was a nervous irritable woman, aged 45, a sufferer from rheumatism. The disease in the breast was discovered by accident, giving no uneasiness for some time; at length occasional darting pains were experienced, extending towards the shoulder. External examination detected partial induration of the gland, extending from the nipple upwards and inwards, the rest of the organ seemed of healthy appearance. Mr. Lawrence extirpated the whole breast in the usual manner. He remarks:]

My object in directing your attention to this case is especially with reference to the question, whether in partial scirrhus of the mammary glands, we should extirpate the entire breast, or only that portion of it which we feel to be affected; and the specimen now before you is one of many which show that the thickness of the coverings, and more especially of the subcutaneous adipose layer, not uncommonly prevents our detecting, until after the removal of the part, changes of structure, which have gone on to a considerable extent at some distance from the original seat of the disease. You here see around the first mass of scirrhous infiltration, which is about equal in size to a large walnut, four or five hard knots, which, though not detected before the operation, present, upon being cut into, those changes which we know to be characteristic of cancer: they are lobules of the mammary gland, connected to the rest of the organs by lactiferous ducts, enveloped in the same capsule, but not so closely aggregated together as in the centre of the organ. Their usually soft structure is converted into a hard semi-transparent light grey substance, in which we see lines and streaks of an opaque yellow hue, indicating the course of some of the tubules, filled with morbid secretion. Precisely the same change is seen in the indurated mass occupying the centre of the gland: the induration becoming slighter, and the alteration of colour less perceptible, as we trace the diseased part towards its circumference, where it blends with the healthy tissues.

There exist, in my opinion, sufficient grounds to lay it down as a rule, that in all cases of cancerous degeneration of the mammary gland, in which an operation is a proper measure, the entire breast should be extirpated. If a portion only be removed, we run the risk of leaving behind some part, which, though small in size, is either equally diseased, as is illustrated by the specimen, or is in a state likely to take on this morbid action, although there may be no cancerous matter absolutely deposited at the time.

In 1847, I was consulted by a lady from whom, a few weeks before I saw her, a portion of the mammary gland had been extirpated for scirrhous disease, by a very careful and well-informed surgeon in France. There is every reason to believe that the operation was performed, as far as it went, in a satisfactory manner; the edges of the wound were brought together, and cicatrisation was nearly complete, when the remaining part of the breast became uneasy. Before five weeks had elapsed, from the date of the operation, the cicatrix became hard and knotty; then the wound re-opened, and the discharge, instead of being purulent, was thin and ichorous. When I saw her there was induration extending along the whole length of the cicatrix; and, towards its inner extremity, there was an irregular ulcerated surface, measuring probably an inch in its long diameter. There was no doubt whatever but that cancerous disease was recommencing in this lady's breast, and that the only measure left her was the removal of all that remained, including the cicatrix. This was done; the wound healed without any unfavourable symptom, and she now, at the expiration of nearly four years, is in the enjoyment of perfect health.

A careful examination, instituted at the time, showed that the dense fibrous structure of the cicatrix was separated and thickened by the infiltration of nucleated cells, which were found in greatest number about the ulcerated spot, and were gradually encroaching upon the deeper structures.—*Med. Times, Nov.* 9, 1850, *p.* 478.

129. —SOME NOTES ON THE ANALOGY BETWEEN SURGICAL FEVER AND PUERPERAL FEVER.

By Dr. J. Y. Simpson, Professor of Midwifery in the University of Edinburgh.

[It is stated that nearly 3,000 mothers die in childbed, every year, in England and Wales, the great majority of which is produced by puerperal fever, Dr. Ferguson believes seven-eighths, though it is more than probable this estimate is too high. Among 2,890 women delivered at the old Edinburgh Lying-in Hospital, 47 maternal deaths occurred; out of these 36 were the consequence of puerperal fever, and the other 11 were produced by other primary obstetric complications and causes.

Dr. Simpson, then, passing on to the consideration of the results of surgical operations, says, that out of every 100 cases of amputation of the limbs, including amputation of the thigh, leg, arm, and forearm, about 30 terminate in death, and 70 in recovery. Among 4,937 cases of amputations, collected from the practice of some of our best civil and military surgeons, by Dr. Fenwick, 1,565 died after the operations, or nearly one in every three. In a large proportion of instances, the death of the surgical patient is the result of a combined febrile and inflammatory morbid state; which he believes to be generically, if not specifically, the same as puerperal fever in the childbed mother, and hence might be denominated "the surgical fever." A few years ago, Dr. Chevers published the apparent causes of death in 153 patients, who had died after surgical operations or injuries in Guy's Hospital. In 134 of these, the *post-mortem* examinations shewed that the more immediate cause of death was—the existence of acute inflammation of one or more internal organs or structures.]

Medical literature does not yet possess a sufficient series of data to enable us to institute a full comparison between all the elements of puerperal and of surgical fever. But the consideration of a few points may prove enough to indicate at least a strong analogy, if not an identity between these two forms of disease. With this view I shall in the following notes attempt very briefly to show in what respects puerperal and surgical fevers are assimilated to each other: 1. In the anatomical conditions and constitutional peculiarities of those who are the subjects of them ; 2. In the pathological nature of the attendant fever; 3. In the morbid lesions respectively left by either disease; and, 4. In the symptoms which accompany each affection.

1. *Analogy in the anatomical, &c., conditions of the subjects of puerperal and surgical fever.*—The anatomical conditions of the puerperal patient after delivery, and of the surgical patient after an operation, are in many respects the same. In the surgical patient we have a wound or solution of continuity on the external part of the body, made by the knife of the surgeon; this wound has, opening upon its free surface, the mouths of numerous arteries and veins; and it comes to be repaired either by the direct adhesion of its opposed surfaces, or, more slowly, by exudation of lymph and pus from its surface, and the ultimate formation of a new skin or new enveloping or connecting tissue. In the puerperal patient we have a wound or solution of continuity on the whole internal surface of the womb made by the separation of the placenta, and the exfoliation of the decidua or mucous membrane of the uterus; this wound has, opening upon its free surface, and specially at the former site of the placenta, the mouths of numerous arteries and veins; and it comes to be repaired under the usual accompanying exudation of serum, lymph, and pus (lochial discharge) from its surface, and by the ultimate formation of a new layer or coating of mucous membrane. The obstetrical patient has the wound complicated with constitutional states of the same kind as those observed in the subjects of surgical operations. Both, at the first, are liable to present the symptoms of shock or collapse, particularly if the labour or the operation has been unusually severe; both have generally a subsequent limited degree of febrile reaction—the traumatic fever of the surgeon—the

so-called milk fever of the obstetrician; and both the external surgical wound and the internal obstetrical wound, are liable to deviate from the standard mode of reparation; for their secretions may alter morbidly; or they may become the seat of an excess of inflammation or of ulceration; or of phlebitic suppuration and its consequences. In the internal obstetrical, as in the external surgical wound, immediately after their infliction, air occasionally enters by the mouths of the veins opening upon their free surface; both are occasionally, though very rarely, followed by delirium, tetanus, and other nervous complications; and, in like manner, but much more frequently, they are apt to be followed by that form of combined febrile and inflammatory action which we term surgical fever in the surgical patient, and puerperal fever in the puerperal patient. In short, the two species of wounds are of the same pathological nature, repaired by the same pathological processes, subject to the same local pathological deviations, and liable to be attended with the same pathological constitutional effects and complications.

[Two opinions were formerly held with respect to the pathological nature of puerperal fever—one class regarding it as an idiopathic or putrid fever, sui generis another class still more earnestly maintaining it to be essentially a local inflammation upon which the fever was depending, and that the malady was treated and cured by venesection and other active antiphlogistics; but most of our best and latest pathologists reject these ideas, and,]

On the other hand, many investigations and experiments made during the last ten or twenty years upon the effects of an acquired, or artificially excited, state of vitiation or poisoning of the blood, have inclined them more and more to adopt the doctrine, that the real source and cause of puerperal fever is to be found in a toxæmia or morbid state of the circulating fluid. The direct injection of pus, and other morbid secretions and matters, into the blood of the lower animals by Gaspard, Cruveilhier, Castelnau, and others, have produced a series of symptoms during life, and a series of lesions on the dead body, showing a very strong analogy to those of puerperal fever. The commixture of pus with the blood in the human subject, in cases of phlebitis, &c., in which pure pus enters directly into the circulation, gives rise to a similarity both of febrile functional lesions as seen during life, and of inflammatory organic lesions as seen after death. And in the puerperal female, there exist such conditions as facilitate the infection of the general circulation, by pus and other morbid matters contained in the uterine cavity. For they may obtain easy access to the general circulation—1. Through the orifices of the utero-placental veins, that open upon the internal surface of the uterus; which are, perhaps, not always completely closed; and which have their mouths constantly in contact with the contents and secretions of the uterine cavity; 2. Through the inoculation of morbid and contagious matters upon the abraded surface of the vagina; and, 3, By any accidental inflammation commencing in the lining membrane of the maternal passages (which are distended and contused during delivery), readily passing by the law of continuity alone, through the venous orifices opening on the interior of the uterus, and thence along the lining membrane of these vessels. Under the now generally adopted view, that puerperal fever originates in a vitiated condition of the blood, we can solve more easily the problem with respect to the relation of the two elements, constituting puerperal fever—namely, first, the febrile action, and, secondly, the internal inflammations, which are present during it. For under this pathological view we see, that the fever is not itself the cause of the attendant inflammations, nor these inflammations themselves the cause of the attendant fever; but that both of them—that is, both the fever and the inflammations—are the simultaneous sequences or effects of one common cause—namely, the original vitiated or diseased condition of the general circulating fluid. And, further, the same doctrine enables us to perceive, how in one set of cases, or one epidemic of puerperal fever, the febrile effect or element may be more marked than the inflammatory; and how, in others, and these generally the most amenable to treatment, the inflammatory element or effect may be more marked and more prominent than the febrile.

These views regarding the pathology of puerperal fever, are borne out and corroborated by what we see in surgical fever and its inflammatory results among the internal viscera of the body; for no pathologist has ventured to maintain that the pluerisy or peritonitis, for instance, which we often see upon the dead bodies of patients who have died of surgical fever, is the effect or consequence of that fever; or that the fever itself is a consequence or effect of these local inflammations. All at the present day are agreed that the phenomena of the occurrence of these local internal inflammations is not explicable, and is not in any way explained by the old doctrine of sympathy or metastasis; and few now dissent from the idea that we are to look for their origin, and for the origin of the attendant fatal fever, in some diseased or vitiated condition of the blood. What this vitiated condition of the blood may specifically and actually consist of, in puerperal and in surgical fevers, —whether it consists in the presence of some one or more of the elements of purulent matter alone, or whether other animal secretions than pus may be its common or occasional cause, and whether the attendant type of fever and its effects may not be fixed and regulated by differences in the diseased material which is present in the circulation, are all questions which a more advanced era of pathological research, and a more subtle chemistry and histology than has yet been brought to bear upon them, will, perhaps, yet, ultimately determine. The character and phenomena of puerperal and surgical fever and their inflammatory results, agree with those of small-pox, measles, scarlatina, rheumatism, and the other so-called "disseminated inflammations" of M. Chomel in this respect, that, whilst originating, as he believes, in different specific "infections of the liquids" or blood, they all agree with each other in several respects; as that they cannot be excited artificially by the common causes of inflammation, but are developed by specific causes; they are thus secondary inflammations resulting from a primary morbid diathesis or alteration of the animal fluids; antiphlogistic measures possess usually comparatively little influence upon the duration of these disseminated inflammations, and often but a doubtful influence upon their intensity; and, lastly, whilst the unity of each affection is preserved, each is represented by a multiplicity of local inflammatory lesions, developed simultaneously or successively, and frequently in organs and parts distant from each other.

[The tables from Chevers, Dugès, and Tonnellé afford a variety of important evidence, and specially illustrate the following points:]

1. Both diseases generally leave upon the dead body ample evidence of the occurrence before death of acute and often extensive internal inflammatory action. 2. The internal inflammatory lesions are seldom limited in the same case to one organ or texture only, but two or more different viscera or surfaces are usually observed to have been either the simultaneous or successive seats of inflammatory action, and the different parts thus attacked are sometimes very distinct and distant from each other. 3. The internal viscera or textures, which are the first and principal seats of inflammation, are often far removed from the original wound or lesion, particularly in those cases in which the wound or lesion is in the head or extremities. From the days of Valsalva and Morgagni downwards, injuries of the head have been observed to be often followed by inflammation and the effusion of pus in the liver, lungs, pleura, &c. Dupuytren, Sir Charles Bell, Guthrie, and others, have long ago remarked that after amputation of the extremities, inflammation of the lungs or pleura (parts sufficiently distant from the seat of the wound) was a very common and a very fatal sequel. Pleurisy was found so often by Velpeau after these and other operations, that he proposed to give the disease the name of "pleurisie purulente des opérés." Rokitanski, Routh, and Kiwisch describe the inflammatory lesions of the chest as very common in the puerperal fever of Vienna and Prague; and I have seen it in some cases in Edinburgh; but generally like many of these internal inflammations in puerperal and surgical fever, the phenomena of it are not very marked during life. This, like the other inflammations in these cases, is often latent in its symptoms, though the post-mortem results show how intense the morbid action had been. But, 4. In obstetric cases the uterus, uterine appendages, and peritoneum, are the most common constant seat of the accompanying internal inflammatory action, though the organs of the thorax, &c., are not unfrequently also affected. Various causes appear to lead to this special localisation of the inflammatory action and effusions in puerperal fever. All kinds of wounds of the pelvic and generative organs are particularly liable to be followed by peritonitis, when they give rise to surgical fever. In midwifery cases the uterus—the seat of the original wound—is, like the external wound in surgical cases, liable to inflame; and this morbid action readily extends, by the law of continuity of tissue, to its appendages and peritoneum.

[Speaking of the analogy in the symptoms of puerperal and surgical fever, Dr. Simpson says there is almost no disease which varies more than puerperal fever does in different cases. And the same remark, with respect to the variability of its forms, &c., holds good with regard to surgical fever. But when either disease is fully marked, the symptoms are sufficiently striking, and similar in each, —the more marked consisting of rigors, a pulse varying in strength, but always constant with regard to the fact of its rapidity; an altered, and frequently darker, or almost icteric hue of the surface; the skin sometimes hot and dry, sometimes bathed with perspiration; local pains, and functional derangements; anxiety and general prostration and adynamia; laboured or hurried respiration; and often, at last, rapid sinking, with or without delirium. Dr. Simpson concludes as follows:]

In the preceding remarks, I have not attempted to discuss fully and minutely all the various points of analogy between puerperal fever and surgical fever. My object has been principally to direct, however imperfectly, the attention of my professional brethren to the subject, under the strong hope, that the future comparative study of the disease, and of its characteristic symptoms, lesions, and causes, in the puerperal and in the surgical patient, may yet serve mutually to illustrate the pathology of this fatal affection in each class of cases; and perhaps this hereafter may lead to the discovery of more enlightened principles of prevention and of treatment, than the isolated and divided study of the same malady respectively by the obstetrician and by the surgeon, has in times past been fortunate enough to elicit. And I do believe that if any man should ever have the good fortune to detect, or suggest any simple or practicable measures, either to avert and prevent, or to mitigate and cure, surgical and puerperal fever, he would, in doing so, confer one of the greatest of all possible benefits upon the advancement of surgery and midwifery, and be the means of saving numerous lives in operative and obstetric practice. The discovery of any such measure or measures, would undoubtedly form a most important era in the march of professional discovery. Nor does it seem utterly hopeless to expect the possible detection of some such measures, in the way of prevention at least, if not in the way of cure. We are the more encouraged to hope for such a result, as we already know various conditions capable of increasing on the one hand, and of decreasing on the other, the changes and the intensity of surgical and puerperal fever. The disease, for example, is confessedly more common and more severe among the population of towns, than among the population of the country; it is more frequent and more fatal among hospital patients than in private practice; and much more so in crowded wards than in those where the patients are few and provided with a full and free supply of fresh air. There are epidemic states in which puerperal and surgical fever is frightfully common; others in which it is very rare and apparently difficult to excite; some localities and towns are far more frequently the seat of it than others are; and various states of the economy seem to predispose the constitution to it, or against it. Surgical and obstetric patients suffering under internal organic diseases (particularly of the abdominal viscera), and under certain functional derangements of the kidney, and perhaps of other organs, seem specially liable to be attacked with this consecutive fever and inflammation. In addition to the common antiphlogistic precautions and measures, various special prophylactic measures have been proposed with the view of guarding patients against attacks of puerperal fever, when epidemic in its character, such as large and repeated doses of quinine, chlorate of potash, &c., before or immediately after delivery. This is a line of inquiry that seems particularly to demand attention, and to deserve further careful investigation and research. Some surgeons have, with a view of rendering their operations more successful, subjected their patients to previous preparations, regimen, and drugging. But the exact power and propriety of these and other measures have by no means yet been precisely fixed and ascertained. Of the propriety and efficacy of one class of prophylactic measures against puerperal fever, few practitioners in this country have any doubt. I allude to those measures which are calculated to prevent the medical practitioner or the nurse being the unhappy medium of carrying the contagion of the disease from one puerperal patient to another. This is not a fit opportunity to enter into such a long question as that relative to the occasional contagion of puerperal fever; nor to enquire under what different circumstances and conditions it may prove capable of being communicated, as whether it ever originates from the mere inhalation of contagious effluvia, brought near to the puerperal patient, &c. I shall content myself with observing here (what I have taught elsewhere for the last ten years), that there exists, I believe, on record, a series of facts amply sufficient to prove this at least, that patients during labour have been and may be locally inoculated with a materies morbi capable of exciting puerperal fever; that this materies morbi is liable to be inoculated into the dilated and abraded lining membrane of the maternal passages during delivery, by the fingers of the attendant; that thus in transferring it from one patient to another, the fingers of the attendant act, as it were, like the ivory points formerly used by some of the early vaccinators; that the materies morbi most capable of being thus inoculated and generating the disease in a new individual seems to be the inflammatory products effected upon the serous or mucous surfaces of females who are suffering under puerperal fever, or who have died of it; and lastly, that other inflammatory effusions, when in the same way transferred and inoculated into the puerperal female, appear to have sometimes the same effect, such as the effusions into tissues, that are the seat of an asthenic, erysipelatous, or gangrenous type of inflammation.

Believing as many practitioners do, in this occasional communicability of puerperal fever, it becomes, of course, their bounden duty to avoid as far as may be, the possible propagation of it in this manner, and to use every available precaution against such a sad misfortune. And the omission of these precautions has proved, I sincerely believe, the cause of many deaths in child-bed, and continues still to do so, particularly on the continent of Europe. In the large hospital at Vienna, out of 21,120 women delivered from 1840 to 1846, 2260 died; or about 1 in every 10 mothers delivered perished, chiefly from puerperal fever. Latterly this mortality has diminished so far, that in 1848, not above 1 in 74 mothers died. This great change was effected, in consequence simply of means being adopted to prevent the contagious inoculation of the disease being carried and transferred by the medical attendants and students, from those already affected and dead of the malady, to women who were in labour. Before 1847, almost every woman delivered in the wards attended by the medical students, was examined by a number of students; and these students had been often allowed immediately previously to touch and handle the bodies of women who had died of puerperal fever, and were even taught upon them the manipulations and operations of midwifery. The mortality altered and diminished immensely and immediately from the time (May 1847), that the assistant physician, Dr. Semelweiss, prevented students from touching parts at the autopsies, and directed all of them to wash their hands in a solution of chlorine before and after every vaginal examination.

In this instance and in others which might be cited, was not the want of a due knowledge of the communicability of puerperal fever, the cause immediately leading to these numerous maternal deaths? Were these mothers not sacrificed merely to medical prejudice, in the form of a total disbelief on the part of our continental brethren, in the contagious communicability of puerperal fever? And, lastly, if puerperal fever may be occasionally communicated by inoculation to puerperal patients, may not surgical fever be occasionally communicated to surgical patients in the same way? The question is perhaps a far more momentous one than the simple past neglect of it might *a priori* lead us to infer. It would be perhaps wandering too far out of my own province if I were venturing to discuss it here. But I may state that, on inquiring into the subject, I have repeatedly heard of instances of a rapid succession of surgical fever cases and disasters in the practice of the same surgeon, while the other surgeons in the same locality had their patients recovering as usual; and I have been told of one eminent surgeon having locked up all his cutting instruments, for some weeks, under the impression that they were in some way or other infected, and unaccountably dangerous to all his patients upon whom, for a short time previously, he had occasion to employ them A gentleman, who was formerly surgeon to a very large hospital, and also an extensive practitioner in midwifery, informs me that, during the period of his surgical superintendence of the hospital, and when consequently often touching the discharges from all kinds of wounds and breaches of surface, puerperal fever was, from time to time, common in his private obstetric practice,—and, at the same period, he saw many of his hospital surgical patients die with similar symptoms. Since giving up the surgical charge of the hospital, the occurrence of puerperal fever has ceased in his private midwifery practice A sufficient series of such cases, would so far add another proof of identity in the two forms of fever—the puerperal and surgical—by proving an identity in their origin and mode of causation.—*Monthly Journal of Med. Science, November,* 1850, *p.* 414.

130.—ON ANÆSTHESIA IN NATURAL PARTURITION.

By Dr. ROBERT BARNES.

[Dr. Barnes says the affirmative of the following question has been too hastily assumed, viz.:]

Does chloroform save the patient from pain and its shock upon the system? Those who contend for the use of presumed anæsthetic agents in parturition were at least bound to substantiate this opinion. I submit that they have signally failed to do so.

In the first place, what mean those groans, that agitation, those suppressed mutterings or open cries which all have observed in patients undergoing surgical operations in the depth of so-called anæsthetic stupor? Are they the expression of suffering, or are they not? Can it be maintained that because patients on awaking express no recollection of that suffering, there was therefore no painful impression conveyed to the sensorium? Will it be said that because perception and memory, especially functions of the waking brain, held under the thraldom of intoxication, could take no cognizance of the fact, that painful impression produced no effect upon the system? In the case of parturition, the unconscious mother is as ignorant of her delivery as of its painful accompaniments. It might as reasonably be urged on that account that the child itself had not been born. Should we be justified in flogging a drunken man acting on the anæsthetic principle, that since he might not *feel* the infliction at the time, or *remember* it afterwards, he could not be hurt? Is there no constitutional injury in this case independent of consciousness? The law of the sequence of shock on the nervous centres upon excitation of the peripheral nerves is as inevitable as the sequence of effect upon cause. I will shortly demonstrate, by some beautiful experiments of Dr. Marshall Hall, which I have had the privilege of witnessing, the mode in which chloroform acts, and some of the relations of shock to the nervous system. It has been erroneously concluded that chloroform simply narcotises the brain, or rather, induces a deep sleep, during which state there is complete unconsciousness of pain, and a consequent avoidance of the shock which pain produces on the system. It is further assumed that chloroformisation carried to the extent which is necessary to subdue pain does not affect the spinal marrow or the sympathetic system. \ strict analysis of the nervous system, aided by experiment and observation will show the fallacy of these conclusions.

Before relating these experiments, I think it necessary to remove an objection sometimes urged by those whose physiological knowledge is insufficient to reach a conviction of the unity of the nervous system in the animal kingdom. It has been said that experiments performed on frogs cannot be applied to conclusions on animals higher in the scale. I beg to observe that the experiments to be cited, or analogous ones, have been repeated with similar results in kittens, dogs, and other animals. That the action of chloroform is identical throughout the animal series no one can doubt. Many will remember Professor Brande's unfortunate guinea-pig, to which chloroform was given with such care, that its speedy recovery was announced to the spectators. At the moment of this confident announcement, the poor creature was dead. The facts recorded in the tables embodied in this paper demonstrate a similar effect on the brain, spinal marrow, and ganglionic system of man, as that observed in frogs. If any doubt remain as to the perfect application of these experiments to the argument, the numerous cases of sudden death from chloroform in the human species will dispel it.

Action of Chloroform.—Experiment 1: *Preparation.*—A frog was placed under a tumbler, having a small sponge attached to its inside, on which ten drops of chloroform had been poured.—*Observations: a.*

The frog at first made strong efforts to escape from its confinement: these efforts became gradually more feeble, and separated by intervals of rest. During the first three minutes, respiration was observed, although it soon gradually diminished in force. At the end of five minutes all motion had ceased; the limbs and head had dropped; there was complete muscular relaxation; the animal seemed dead; cerebral system and voluntary function affected. -*b*. On being removed from the glass, and pinched with the forceps, *no diastaltic movements* were produced; the spinal system as well as the cerebral was overpowered.—*c*. On placing a foot under the microscope, the circulation was observed to be extremely languid; the ganglionic system was also affected.—*d*. Yet the frog was not dead. It was placed in cold water, and in an hour afterwards it recovered; its recovery, however, was an unusual circumstance. Dr. Marshall Hall, who has repeated this experiment many times, has found chloroform a far more rapid and deadly poison to the frog than prussic acid.

We see, then, that the action of chloroform is not restricted to the cerebral organs; it extends to, and paralyses, the spinal cord, and even the ganglionic system. And although it is probable that it does not affect these three systems in all cases with equal facility, yet there is no proof that we can give chloroform with such nicety as to confine its action to the brain, that we can stop short of affecting the spinal cord or ganglionic system. On the contrary, even when given in moderate doses, it has often been found that every part of the nervous system has been affected at the same time; and numerous cases of fatal syncope taking the practitioner by surprise, have proved that the ganglionic system may be irrecoverably struck simultaneously with the brain.

Exp. 2: *Preparation.*—A frog (male). The brain was entirely removed; then the abdominal and thoracic viscera were taken away. Thus, the cerebral centre and the ganglionic system being abstracted, the spinal system alone remained of the nervous apparatus. -*Observations:* *a*. On pinching a foot, the skin being intact, vigorous diastaltic movements were produced. If these pinchings were repeated frequently the diastaltic movements gradually diminished in vigour, and after a short time ceased: there was *exhaustion from shock*. *b*. After an interval of rest, the diastaltic movements could be again excited: there was recovery from shock and exhaustion. *c*. When the pinchings were still continued, after a time no farther diastaltic movements could be excited: there was complete exhaustion of the diastaltic function.

If any further proof be wanting to show that this exhaustion of the diastaltic function is the result of over-excitation or shock, it will be found in the fact that a second frog similarly prepared, but not subjected to such repeated excitation, will retain the activity of the diastaltic function, long after the spinal marrow of the first frog is irrecoverably dead.

Exp. 3: *Preparation.*—As in the former case, the brain and viscera of the abdomen and thorax were removed. In addition, the skin of one foot was peeled off; thus removing the extremities of the *esodic* nerves. *Observations:* *a*. On pinching the foot from which the skin was peeled off, no motion was produced. The *commencement* of the diastaltic nervous arc was wanting. *b*. On irritating the spinal marrow with a needle, convulsive movements of all the limbs were produced. When the spinal marrow was completely broken up, no further movements could be excited even by irritating the foot on which the skin was left entire. In this case, the *centre* of the diastaltic nervous arc was wanting. A breach in any part of the diastaltic nervous arc puts an end to the manifestation of the diastaltic function.

Action of Strychnine.—I refer to the admirable Croonian Lectures of Dr. Marshall Hall, recently published in 'The Lancet,' for another experiment, which strikingly elucidates the influence of shock.

Exp. 4: *Preparation.*—The tenth part of a grain of the acetate of strychnine, dissolved in distilled water, was applied over the cutaneous surface of a frog.—*Observations*: *a*. The most extreme excitability or hyperethism was induced. The slightest stimulus caused violent tetanoid spasm. *b*. It is the result of many observations, that "a frog so affected by strychnine *recovers*, if it be placed in a little cool water, and left *absolutely undisturbed* in a cool place! It *dies speedily*, if continually stimulated, even by the touch of a feather. Quiescence cures, whilst each excited tetanoid spasm exhausts the vital power!"

What is the irresistible lesson drawn from these remarkable experiments? That irritation of the *esodic* nerves is surely transmitted to the spinal centre where it produces its shock. If this irritation be too long repeated, the vis nervosa is surely exhausted. And, *lastly*, that exhaustion of the vital power surely results from over-excitation of the diastaltic function, (we may call it shock,) even when the brain is unaffected, as by strychnine, or when it is entirely removed, as in the second experiment! Shall narcotisation of the brain avail?

In parturition as in surgery, in physiology as in pathology, the physical shock of nervous excitation or injury cannot be escaped from. In parturition as in surgery, the emotional shock of nervous excitation or injury may be avoided. In surgery this may be an invaluable boon. Is it so in parturition? It is more than doubtful. The emotional influences, both those excited by the sensation of pain, and those independent of pain, are of immense importance in this complicated function. The voluntary power, too, is often of the highest utility. In woman, at least, the brain should be allowed to retain its integrity, to exert its beneficial controlling influence over her own conduct, as well as its salutary aid in attaining a safe delivery; and, if for nothing else, to mark, that in her, parturition is something more than a mere animal act!

We cannot escape the conclusion, that the narcotism of chloroform is replete with mischief and danger,—whether it simply arrest the function of the brain, rendering it insensible to painful impressions, and depriving the system of that emotional safeguard which opens the glottis in the acme of the expulsive effort; or whether it paralyze the peripheral nervous system, cutting off all painful and diastaltic excitations at their source.—*Lancet, July* 20, 1850, *p*. 82.

131.—*Two Cases of Ruptured Perineum, with Operation.* By W. FERGUSSON, Esq., Surgeon to King's College Hospital, &c. [After dwelling on the difficulty of accomplishing the re-union of the parts in ruptured perineum, Mr. Fergusson says, that "it is generally allowed that in cases which do not heal up soon after delivery, the parts being kept in perfect rest it is better to wait, before attempting any surgical means, until the primary inflammation has subsided. Mr. Fergusson has chosen the interrupted suture, certain objections to them being removed by precautionary measures. The first patient, 18 years of age, had been delivered of her first child by the forceps six months previous to her admission, and ever since had been suffering from the effects of a lacerated perineum, extending from the fourchette to the margin of the anus, the fæces involuntary passing through the recto vaginal aperture.]

After this young woman had been duly prepared for the operation, by rest and opening medicine, she was brought into the theatre, June 1st, and placed under the influence of chloroform. The patient being secured in the position for the operation of lithotomy, Mr. Fergusson began by paring the whole length of the margins of the perineal gap, from above downward, and brought them in exact apposition, by three interrupted sutures. The next step of the operation consisted in making a longitudinal incision on each side of the line of sutures, in order to take off the tension which the neighbouring parts would naturally exercise upon the sutures. These lateral wounds were filled with dry lint, cold compresses applied to the perineum, and the patient removed to her bed, with her knees kept together by a roller.

Much care was subsequently used in keeping her quiet, and regulating the action of the bowels by enemata; and about eleven days after the operation, Mr. Fergusson was enabled to remove the three sutures, as pretty firm adhesion had taken place. The bowels now became regular, without the necessity of the injections which had hitherto been systematically administered, and about one month after the operation, both the perineal wound and the lateral incisions were completely healed. The further progress of the patient has been satisfactory, the rectum has regained the faculty of retaining its contents, and the poor woman is delivered from a very distressing infirmity.

The second case refers to a woman forty-two years of age, who has been married sixteen years, has had four children and one miscarriage. In her last confinement the presentation was transverse, and it would appear that, though the delivery was effected without the use of instruments, the perineum was severely lacerated. Besides the inconvenience attending this accident, she suffered likewise from a very usual consequence of it—viz., prolapse of the uterus.

When this patient was admitted under the care of Mr. Fergusson, in March, 1850, it was discovered that, besides the derangement of her generative organs, she was tormented with tape-worm, and she expressed great anxiety to be freed from the unwelcome guest. She was therefore transferred to the physicians' ward, where, by the agency of the kousso, she evacuated several yards of the tænia.

When the patient had returned to the surgical ward, Mr. Fergusson began the treatment by cauterizing the walls of the vagina, whilst the uterus was in a prolapsed condition, about one inch below the os uteri. He then replaced the womb in its natural position, and directions were given for injections of warm water to be thrown up every second hour. Patient went on tolerably well for about a fortnight, when the discharge from the vagina became rather abundant, and the womb again prolapsed with each evacuation of the bowels, or even when she turned in bed. The patient now began to complain of shooting pain in the left groin, and as the uterus still continued to come down, upon the slightest exertion, Mr. Fergusson resolved to delay the final operation no longer, and on the 1st of June the patient was brought into the theatre.

She was placed in the position for lithotomy, like the subject of the preceding case; and having been rendered insensible by chloroform, Mr. Fergusson pared the edges of the perineal wound, which extended from the fourchette to the anal orifice, and for a short distance through the recto-vaginal septum, and having brought them together, they were secured by three interrupted sutures, the lateral incisions were made, and the parts dressed as in the former case.

The patient was kept very quiet, her legs being kept together by a roller, and her bowels remained confined for six days after the operation. Some pain in the abdomen, numbness of the lower extremities, and difficulty in passing the motions, were for some time complained of, but the sutures came away successively, without any untoward circumstance, and eighteen days after the operation, the parts were perfectly united, except a small opening at the anterior portion of the wound. On the 27th of June, twenty-seven days after the application of the sutures, the whole perineal wound was perfectly united, so much so, that the linear cicatrix could hardly be seen; the two lateral incisions were likewise closed up; but when Mr. Fergusson examined the parts, a few days afterwards, the vagina was found tinged with fæcal matter, and it was discovered that a small communication existed between that canal and the rectum. Mr. Fergusson considered that this aperture would soon close, without any need of cauterization, as for the last fortnight the escape of fæcal matter into the vagina had gradually been lessening.—*Lancet, July* 20, 1850, *p.* 93.

132.—*On an Improved Method of Making Sponge Tents.* By CHARLES COATES, Esq., Leeds.—Allow me to suggest an improvement on Professor Simpson's method of making sponge tents for internal polypi.

Dr. Simpson's tents are made by dipping a piece of sponge in a strong solution of gum, tying and compressing it around a central wire, as the axis, into the required conical form, by a continuous layer of whipcord, drying it, removing the cord, and subsequently coating the surface of the tent with tallow or axunge and wax.

The plan I adopt is to select a piece of the best cupped sponge, of the required form and size, and, after beating and washing it well to free it from any sand or gritty particles it may contain, to squeeze it almost to dryness in a towel. A piece of tape about ten inches long is then passed through the sponge at about one-third from its base, making a shallow groove on each side from the apertures to the base for the tape to rest in, both ends of which are tied together for the withdrawal of the tent when necessary. Having fixed one end of a piece of whipcord (say two or three yards long) to some firm object, I commence winding the other end on the sponge, gradually, from apex to base, as firmly us possible. The tent is then well dried before the fire, or in an oven, previous to the removal of the cord. When it is thoroughly dry, a small hole about half an inch deep should be made in the base, with a joiner's pricker or a heated wire, for the introduction by the director. The whole surface of the tent is then slightly coated by dipping it into equal parts of melted lard and bees-wax.

The advantages of this plan are obvious:

1. The tents are made with much greater facility.
2. It permits of their being compressed into much smaller compass.
3. The expansion of the tents is equally gradual, and never requires the aid of "tepid water injections," the moisture of the surrounding parts being quite sufficient.

No possible good can accrue from steeping the sponge in a solution of gum, which not only renders the operation most disagreeable and uncleanly, but also prevents the close contraction by the retention of the glutinous particles in the interstices of the sponge.

[We have seen specimens of these tents of Mr. Coates, and can confidently recommend them to the profession. Dr. Simpson himself will, we are sure, be pleased to acknowledge an improvement on his own method.]

133.—*On Uterine Neuralgia.* By M. VALLEIX.—[The following valuable remarks on uterine neuralgia, by M. Valleix, appeared in 'L'Union Médicale:']

M. Valleix has pointed out a class of cases in which the cervix uteri, usually insensible, becomes the seat of the most acute suffering of a neuralgic nature, and has pointed out the diagnostic distinctions between these and cases of painful inflammatory congestion of that organ.

This neuralgic form of disease is frequently met with, but is not often recognized. In all the cases which have come under M. Valleix's notice, the affection has been accompanied by pain following the course of the lumbo-abdominal nerves. He therefore regards the neuralgia of the cervix as part of the more extensive lumbo-abdominal neuralgia. This feature is of importance in reference to the diagnosis of neuralgic from other pains of the cervix uteri.

The subjects of this neuralgic affection present all the characters of sufferers from chronic uterine congestion, with which disease the former is almost always confounded. The pain is much augmented at the menstrual period, producing dysmenorrhœa. The vaginal discharge which occurs in these cases is analogous to the increased secretion from the mucous membrane of the eyelids in certain affections of the trifacial nerve. They are both functional disturbances originating in disordered nervous influence.

Careful exploration is of the first importance to its detection. The neck of the uterus will be found tender to the touch, often so to a great degree, the tenderness being most acute at the sides of the cervix, while the anterior and posterior surfaces are free. The cervix is of its ordinary form and size.

On examining the abdomen and loins, a neuralgic pain will be detected in the hypogastrium a little beyond the middle line on one side most frequently the left only. Along the course of the first pair of lumbar nerves will be discovered other points, more or less acutely painful, and more or less isolated. The painful point always corresponds with the seat of pain in the uterus.

The chief diagnostic features are, the degree and isolation of the tenderness, the intermittent character of the pain, and the occurrence of the neuralgia of the abdomen and loins.

The treatment adopted by M. Valleix has been blisters to the hypogastric region, cauterization of the cervix, and narcotic injections, with absolute rest and general treatment. *Medical Gazette, July 6, 185[illegible], p.* 43.

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134. *Obstructed Vagina, with Retention of the Menstrual Discharge.* By Professor SYME. A. D., aged 19, was admitted on the 10th of February. It was stated that, twelve months before, she had suffered two severe attacks of hematemesis, and had been confined to bed for two months by the weakness so occasioned. About five months before the time of admission, she noticed a tumour in the lower part of the abdomen. It was the size of a fist, firm, and seated above the pubes, rather inclining to the left side. Since then she had complained of pain in the back and lower part of the belly, with regular monthly exacerbations of these symptoms, and at each period had observed a distinct increase in the size of the swelling.

Upon examination it was found that the orifice of the vagina was obstructed by a dense thick membrane, through which a fluctuation on the inner side could be distinctly perceived. The abdominal tumour in size, situation, and consistence, resembled the uterus about the sixth or seventh month of pregnancy, and at the left side had a projection, somewhat larger than an egg, which seemed to be composed of a firmer substance. Dr. Simpson, who had the kindness to see the patient at my desire, thought that this tumour might depend upon a fibrous growth of the uterus; but did not regard it as affording an objection to evacuation of the retained discharge, which unduly distended the organ.

On the 12th, I pushed a bistoury through the obstructing membrane, which was extremely thick and tough, and then guided the knife on my finger so as to enlarge the opening to the full extent of the vagina. Pressure being now made on the distended uterus, induced the contents to issue in the form of a semiconsistent stream of reddish brown coloured matter, to the amount of at least four pounds. When no more could be pressed out, injections of water were thrown in both then and frequently afterwards, so as to wash out the remains of the accumulation and also the discharge, which continued for several days. Under this treatment the patient quickly recovered without any unplesant symptom. The os uteri, which at the time of the operation was felt to be dilated as it is after parturition, soon contracted; all trace of the abdominal swelling disappeared; and the patient was dismissed well on the 7th of March.

The point of interest in this case seems to be the illustration which it affords of the advantage attending a very free division of the obstructing membrane, and the use of syringing with water subsequently. In similar cases a typhoid fever of alarming degree, and even death, has resulted from punctures made with a view to evacuation; and I feel indebted to the professor of midwifery for impressing upon me the importance of preventing any of the impurities from being retained. The knife is certainly much preferable to a trocar, and should be used with such freedom as to insure complete division of the dense membrane, which effectually resists the finger, so as not to admit of being torn.—*Monthly Journal of Med. Science, August,* 1850, *p.* 147.

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135. *Intestinal Obstruction giving rise to Symptoms Simulating the Pains of Labour.* By Dr. ALEXANDER KNOX, Strangford, County Down, Ireland.—[This was a case in which Dr. Knox was sent for on account of a supposed "stoppage" in the intestinal canal, although heated and feverish, the painful, periodical, violent expulsatory efforts closely resembled labour. The history of the case and the character of the patient prevented this conclusion being arrived at; and the symptoms, with the enlarged but not tympanitic abdomen, and the feeling of solidity which it possessed, led the Doctor to form the opinion, that some solid body was impacted in the alimentary canal, inducing reflected excitement in the abdominal muscles. Accordingly, says he,]

Having oiled my hand, I introduced it as far as possible into the anus, for the purpose of exploration, when I detected some firm bodies completely plugging up the rectum, and succeeded in removing a number of them, some entire, and others broken down by the fingers and the shank of an iron spoon, in the process of extraction. They were of a spheroid form, and each about the size of a small apple, and of a deep grass-green colour, and firm tenacious consistence. Having hooked out as much of the entire mass as I could with my fingers, to the great and immediate relief of the patient, I administered a large emollient injection with Read's syringe, which was then perfectly effectual in unloading the lower bowels, although it had produced no effect when previously employed. A course of enemata and gentle aperients left the patient, within a week, perfectly restored to health. I naturally felt curious to ascertain the composition of these green balls, to which I had never seen anything similar. I found them, by breaking one or two of them down on a clean towel, to consist entirely of green vegetable matter, firmly agglutinated with the natural mucous excretion of the intestines. When this connecting medium was removed by frequent ablution, the entire residue was found to consist of the fibres and minute fragments of a green vegetable substance, which, on farther examination and inquiry, I found to be that of the early cabbage, which had then had a little serum in the breasts, at the time when the use of the Bofareira was commenced. The secretion speedily became milky. This patient had a leucorrhœa, which had been present ever since the weaning; and the uterine and vaginal irritation, upon which the leucorrhœa depended, had kept up, in all probability, the serous secretion from the breasts, which is common enough in leucorrhœal cases.

Since the foregoing cases occurred, I have used the remedy in a case of scanty menstruation of a remarkable kind. Owing to exposure to marsh malaria, some years ago, the patient had scarcely a sign of coloured discharge at the usual catamenial periods. She used the infusion of the leaves of the red Bofareira at the date of her period, applying the infusion and leaves to the breasts, and the vapour to the genitals, with the effect of producing, in two days, a considerable flow of the catamenia. From the effects in this case, and in one of the cases already related, the Bofareira promises to be of considerable value as a direct emmenagogue; at all events the cases in which I have tried it, show that the plant does not lose its efficacy in this climate. I hope that, in America and other parts in which the plant is common, perennial instead of annual, extensive trials of its efficacy both as an emmenagogue and a galactagogue, will be made.—*Lond. Journal of Med., Oct.* 1850, *p.* 951.

143.—*On a Japanese Remedy for Sterility.* By Dr. E. WILLIAMS.—[Dr. Williams believes there is a botanical remedy in Japan and China which exerts a specific influence upon the uterus, more particularly in cases of sterility and checked menstruation. The tree is one of the order Ternstromaceæ of Jussieu, growing to the size of the English laurel, with leaves somewhat larger than the congou tea, and the odour resembling pulegium and sabina.]

The mode of preparation is to take a quantity of the leaves, macerate them in as much rice-spirit (samshu) as will just moisten them for six hours; then express and give about a teaspoonful every hour, and two or three doses will invariably bring on the menstrual secretion, which can be maintained by a dose or two daily for any length of time.

Females in the healthy state are expected to menstruate in their eighth year in Japan and China, and should they not do so they are ineligible for betrothal; therefore recourse is had to the *key tu sing* with certain results.

To ensure its success, according to popular belief, the leaves must be gathered by a virgin the while using certain cabalistic formulæ, at the full of the moon, and during the burning of a certain number of highly perfumed jop-sticks.

When required for the purpose of obviating sterility, the tree must be in its second year, and also be gathered with certain prescribed formulæ at the wane of the moon, and equal parts of the root must be added to the preparation, which is made in the same manner as the preceding receipt.

That the root is aphrodisiac in its effects I have not the slightest doubt, as I have administered it to animals with obvious results, and without any ill effects even to mules and castrati.—*Lancet, September* 14, 1850, *p.* 321.

among these, in many cases, I believe the alternate use of the hot and cold douches will be found to be very valuable, especially if combined with stimulants judiciously varied and skilfully administered externally and internally. In many cases, repose, dashing cold water in the face, keeping the surface warm, and TIME may be the only means which ought to be used.

[He then proceeds to the statement of facts proving that the entrance of air by the open mouths of the uterine veins may cause dangerous symptoms, and even death; and to the history and critical examination of some of the alleged cases.]

Legallois, in 1829, when experimenting upon animals, with a view to elucidate the effects of loss of blood, and abstinence, during gestation, observed, in three different cases, that air penetrated into the vena cava inferior and uterine veins; and that this was followed by instantaneous death. He was naturally much struck with the coincidence; and properly connecting the two circumstances as cause and effect, he has asked, in the memoir which contains the experiments referred to, whether, in some of those sudden and unexpected deaths which have occurred after delivery, and in which no explanatory lesions have been found on dissection, the entrance of air into the veins has not been the cause of death? Ollivier has also suggested a similar explanation of certain cases of sudden death after parturition. In my Graduation Thesis, published at Edinburgh in 1837, in noticing these opinions of Legallois and Ollivier, I made the following remarks—"There are various circumstances which render it possible, that in some instances in which women die unexpectedly after parturition, when all seems to be going on well, death is owing to air entering the circulation by means of the open mouths of the veins communicating with the uterine sinuses. These orifices, immediately after the separation of the decidua, are very large. They have been made the subject of investigation by many modern as well as old anatomists and obstetricians; and, upon the whole, the various descriptions correspond. Burton (writing in 1751) says, that the uterine sinuses in the ninth month of gravidation are so large as to admit the end of the biggest finger; and their orifices that open into the cavity of the womb will at the same time admit the end of the little finger. (New System of Midwifery, p. 19, Ed. 1751.) Now, the uterus not unfrequently contracts and expands alternately with considerable energy after the expulsion of the foetus; and it is quite reasonable to suppose, that air may sometimes be sucked into the gaping mouths of the uterine vessels, in sufficient quantity to prove fatal to a woman exhausted with the fatigues of labour."

Thirteen years have elapsed since the Thesis now quoted from was published: but, though during that period numerous systematic treatises on Obstetrics have appeared, I am not aware that in any one of them even a single line has been devoted to this subject. Being convinced that, notwithstanding this remarkable neglect, the subject is one of great practical importance, as well as of deep scientific interest, I again bring it before the notice of the profession, trusting that the meagreness of the information to be communicated may be largely supplemented by others, of ampler experience, to whom the following considerations are respectfully addressed.

Many authors refer to this sucking in of air by the uterus, without however in any way connecting it with the immediate subject of this paper; but it may be sufficient to give a single quotation from a recent work. The following passage is from Dr. Charles Meig's 'Letters to his Class, on Females and their Diseases.' "I have," he says, "often been present at the discharge of large quantities of gas from the genitalia. A woman shall be seized with her first labour-pains, and, bearing down with great violence, thrust not the child only, but the placenta also forth upon the bed; and in bearing down with the violent force of the labour-tenesmus, she pushes the very womb itself to the bottom of the pelvis, shortening the vagina in so doing, wrinkling and crushing it down to the os magnum. As soon as the tenesmus is over, the resiliency or elasticity of the tissues recovering its power, the womb rises again to a certain height within the excavation of the pelvis; but, as it is a cul-de-sac that rises, it is natural for air to follow it, and the vagina and the womb itself may thus contain air that has been drawn up within them, upon the same principle as that which makes it follow the movement of a piston in a cylinder. Then comes a new pain—an after pain; or else I may apply my hand to the hypogastrium, and make sure of a good contraction of the womb. If I compress the womb with my hand, and particularly if I push it downwards in the pelvis, I am very apt to cause a quantity of air to rush out at the ostium vaginæ, with considerable noise. This I have heard a great many times. So, in making the examination *per vaginam*, when the uterus is very low down, or when, in making use of the speculum, I push the os tincæ far away from the os magnum, air enters the passage, and follows the retreating womb. If it be left there, and the woman be seized with a fit of coughing, or if she move quickly, or change her position, the air is pressed out with the sound of the *pet-vaginal.* There is no other way to account for it that is reasonable. So, also, a woman has a heavy womb, with a large loose vagina; she lies on the couch or bed; and the uterus retreats, as La Motte says, drawing air after it; but she rises, or coughs, or turns, and the air is expelled." (p. 289).

If it be admitted, that air is drawn into the uterus in the way described, what ground is there for supposing that it may thence pass by the uterine veins into the vena cava, and so on to the right auricle of the heart? The uterine veins, it must be remembered, are large canals, some of them with open mouths, sufficient to admit the little finger. Nature generally closes them firmly by a coagulum; but an accession of hemorrhage may dislodge this; or the coagulum may contract and fall out, and thus leave the orifices of the veins patent. This accident may occur hours, days, or in some exceptional cases, even weeks after delivery.

Of the freedom of the communication between the cavity of the womb after delivery, and the vena cava inferior, any one may be informed by the most cursory examination of the parts on the dead body of a puerperal woman. Dance says, in his essay on phlebitis, "If even a thick injection be thrown into the vena cava inferior below the emulgent veins, it is immediately poured forth in abundance into the cavity of the womb, and overflows from the vulva; thus proving, that after delivery, the large uterine veins are open in the uterine cavity, and communicate freely with the large abdominal veins.

It might, therefore, be anticipated, *a priori*, that air might get into the vena cava, and so onward to the heart of a woman recently delivered, by being sucked into the uterus by the sudden dilatation of that organ, and by thence passing into the orifices of the uterine veins. When the uterus is relaxed, these large openings are gaping, the structure of the parts effectually preventing apposition of their parietes. Unless closed by a plug of coagulum, they are (as I have satisfied myself by examination) perfectly patent. If any obstacle exist,—such as a large clot, or contraction of the circular fibres of the cervix, or of the sphincter of the os uteri, after the air has rushed into the relaxed uterus, it must, of necessity, be propelled into these openings, should the body of the uterus contract without being able to force the air through the os. I have repeatedly seen the womb, after delivery, become inflated with air; and when the organ has contracted, I have heard the air hissing through the vulva. I have, therefore, not only no difficulty in believing, but am constrained to admit, that, should any impediment be offered, in such cases, to the free exit of the air by the os uteri, it must be forced into the uterine veins, were their mouths not protected by coagula; and thence it would rapidly pass by the current of the circulation, up the vena cava into the right auricle.

But, that cases of this kind *may* occur is not all. They have occurred; though, from attention not having been sufficiently drawn to the possibility of such accidents, fewer may have been recognized than have actually occurred. At the same time, it seems probable that the accident is a very rare one.

Dr. Collins, of Dublin, Dr. Murphy, of London, and others, long in extensive practice, inform me that they have never seen such an occurrence as the death of a woman from the entrance of air into the uterine veins. Dr. Lever, however, who views the matter in a different light, has addressed to me the following letter:

[Dr. Lever commences his letter by stating that, as his notes of the cases were unfortunately lost, he is obliged to trust entirely to memory. He proceeds:]

"All the women were multiparæ; the labours had not been over-tedious; the placentæ were thrown off without assistance; and an inert state of the uterus, with hemorrhage, ensued. This viscus seemed like a softened India-rubber bottle; and the period at which the air was *supposed* to have entered it, was when the hand was withdrawn from the abdomen, after employing forcible compression. There was an accompanying noise, very similar to that heard when a caoutchouc enema bottle is squeezed, and then allowed to fill with air; in fine, I may describe it as a rushing sound. All three patients died speedily; the quantity of blood lost not being, in my opinion, sufficient to account for death. Two were examined; one in cold weather, the other in the very height of summer. The latter presented signs of commencing putrefaction. In neither was there any large quantity of blood in the uterus; but in the sinuses, uterine veins, spermatic veins, and left renal vein, there was indubitable proof of air. This, in the former case, emitted no offensive smell when liberated. All these women had been exposed to marsh malaria, and all had enlarged spleen."

Professor Simpson has communicated the following remarks, as an appendix to Dr. John Reid's *Essay on Death from the Entrance of Air into the Veins*:—

"Several years ago, I saw," says Professor Simpson, "a case of death a few hours after delivery, which first gave me the idea that death may sometimes occur from the introduction of air into the uterine veins. The patient was an inmate of the Lying-in Hospital, and was delivered of twins. Dr. Zeigler was called to see her, in consequence of some difficulty connected with the birth of the second child. Post-partum hemorrhage, with alternate contractions and relaxations of the uterus supervened; and she seemed to rally very imperfectly from the effects of the flooding. I saw her in an hour or two afterwards. She had then a very weak and rapid, almost imperceptible pulse; an extremely anxious countenance; and here and there was an evanescent scarlatinoid rash over the surface of the body. The body was opened a few hours after death, as we were anxious to know if there was air in the veins; and we were, of course, desirous not to incur the fallacy of air arising from decomposition. The abdominal contents were exposed under water. The uterine and hypogastric veins, and lower vena cava, were full of frothy blood, and the air bubbled up. The large veins of the extremities were in the same state."

Nothing is said of the condition of the heart and lungs; the only organs in which the characteristic and certain evidence of the accident having caused death is to be found. The mere presence of air is no proof that it has caused death.

Dr. Simpson has the following remarks upon the mechanism of the introduction of air in such cases. "I think we can understand it, when we remember that the interior of the uterus after delivery, especially opposite the late seat of the placenta, is studded with venous orifices; and that, if air does once become introduced into the uterine cavity, from relaxation of the walls of the organ, it will be liable to be *forced* into these orifices, and hence into the general venous circulation, provided the uterus, in again contracting, is unable to expel its contents through the os uteri."

The cases and comments of Professor Simpson are chiefly valuable as an intimation from so eminent an authority that the ideas of Legallois, as to the reality of this accident, deserve credit. The cases may or may not have been, though they probably were, examples of the accident. It must be felt that the descriptions are not absolutely convincing; for the symptoms during life are not sufficiently detailed, and the frothy blood in the uterine veins, hypogastric veins, and vena cava inferior, can with difficulty be understood, except by supposing that, notwithstanding the short time which had elapsed since death, when the body was examined, some decomposition of the blood, and evolution of gases, had commenced. In experiments on animals, and in cases in the human subject, in which rapid death has followed the introduction of air into the veins, frothy blood has only been found in the right side of the heart and pulmonary artery: in other parts of the body, the air, if found all, has not been intimately mixed with the blood, but has existed in distinct bubbles. This is easily understood; for it is the churning of the struggling heart, unable to propel its elastic contents along the pulmonary artery, which causes the frothing of the blood.

It would be endless to relate all the cases which might be cited as furnishing a strong probability that air had entered the uterine veins after delivery, inducing, in some instances, transient, but alarming symptoms; in others, occasioning almost mortal effects; and in others, causing, or along with uterine hemorrhage, contributing to cause sudden and unexpected death. Such cases would not furnish data of sufficient accuracy to support, far less on which to found, conclusions.

Deductions as to the prevention and treatment of accidents arising from the entrance of air by the uterine veins, after parturition.—From the facts already stated, it is plain that the treatment, both preventive and curative, is to obtain natural and permanent contraction of the uterus after delivery. As it is extremely probable that loss of blood, and the entrance of air, in many cases conjointly cause death, it is satisfactory to feel assured that the proper treatment for the one is the best also for the other, so far as the one thing primarily essential is to strive to get the uterus to contract. Plugging will also be specially proper when there is convulsive contraction and expansion of the uterus, with or without much hemorrhage; for then there exists the greatest aptitude for the atmospheric air to enter, or be forced into the uterine veins.

If a large quantity of air have entered the circulation, unequivocal evidence of this will be found by listening to the heart, when the churning sound will be heard. If death does not almost at once close the scene, the phenomena of asphyxia will set in; their rapidity and violence depending on the quantity of air which has entered, on its passing up to the heart in one large volume, or in divided quantities, on the presence or absence of hemorrhage, and on the strength of the patient. The distension of the right side of the heart, and the present or impending asphyxia, must be met by an application of the general rules given at the close of the first section of this paper, so far as they are consistent with the special circumstances of each case. In fact, there is no uniform routine practice which can be rationally recommended in this formidable class of cases. If the practitioner understand the nature of the accident, he will be able to adapt his remedial measures to the emergency. The special means adopted may infinitely vary (as in uterine hemorrhage); but the general principles of treatment are immutable, and simple, and ought to be engraven in the mind of every accoucheur. The amount of success attending their application must depend upon the promptness, nerve, and sagacity of the practitioner.—*London Journal of Medicine, October,* 1850, *p.* 928.

137.—SPURIOUS PREGNANCY—ITS FREQUENCY AND NATURE.

By Dr. Simpson and Dr. Keiller.

Dr. Simpson first showed how frequent and marked the phenomena of spurious pregnancy were among domestic animals, as in the bitch, cow, &c. In these animals the phenomena of pregnancy often occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, but during which there was no intercourse with the male. Occasionally the phenomena of pregnancy in them were suddenly interrupted, and terminated a short time after their commencement; sometimes, however, they went on increasing to the full term of pregnancy, and ended in a series of puerperal phenomena, as the presence of milk in the mammæ, &c &c. Dr. Simpson adduced a variety of facts, showing the great frequency with which the same phenomena of spurious pregnancy occur in the human female in a more or less marked degree, and how these phenomena are often mistaken for dyspepsia, hysteria, and various anomalous and perplexing ailments. And as these phenomena sometimes occur in the virgin state among the lower animals, so do we find them also sometimes occurring in the unmarried human female, and giving rise to symptoms that have hitherto defied any nosological arrangement. Lastly, he spoke of the great use of aperients combined with tonics, and with nux vomica and galbanum in the subjugation of the phenomena of spurious pregnancy.

Spurious Pregnancy, followed by Spurious Parturition.—Dr. Keiller communicated the particulars of a case in which the symptoms of spurious pregnancy, and subsequently those of spurious parturition, existed to such a remarkable degree, as to induce the patient and her friends not only to prepare for the expected accouchement, but, when the supposed full time arrived, to believe in the actual commencement and continuance of a very painful labour, which ultimately became so protracted as to demand, according to the opinion of the attending accoucheur, the immediate and unavoidable performance of the *Cæsarian section!*

Dr. K., having been requested to visit the case for the purpose of satisfying the friends of the patient as to the propriety of having recourse to such an extreme mode of accomplishing the delivery as that which had been seriously proposed to them by the individual in attendance, was astonished to find, on making an examination, without any misgivings as to the parturient condition of the patient, that all the supposed symptoms of pregnancy and of parturition had been and were still, *entirely spurious,* the uterus being evidently unimpregnated!

This startling opinion was anything but credited at the time by the females present, who altogether ridiculed the idea of the case not being one of "*real labour,*" as the motions of the child had long been not only distinctly felt, but even seen "through the walls of the much distended abdomen," the patient herself insisting that the child's movements were so violent that she felt "as if it would leap through her side."

The result of the case, however, sufficiently proved that her painful attempts at delivery could not possibly have been rendered less futile by the *Cæsarian section,* or any other *obstetrical* aid, as *pregnancy never had existed!*

The patient remained for a considerable time afterwards under the immediate care of Dr. Keiller, who stated that he considered the entire group of anomalous symptoms (which she presented in a very singular degree, and which he purposes recording) in a great measure referable to hysteria.—*Monthly Journal, July,* 1150, *p.* 90.

138.—*Application of Cold as an Anæsthetic Agent in Operations for Removing Warty Excrescences.* By THOMAS W. NUNN, Esq., Surgeon to the Western Dispensary.—[Mr. Nunn says, that having taken advantage of the hint given by Dr. Arnott, he has been so satisfied with the result that he considers it due to that gentleman to publish the following case. It occurred in a young married woman, who applied to the Dispensary for the purpose of being relieved of a large accumulation of warty growths about the pudenda.]

The excrescences depended from the whole of the labia minora, and surrounded the clitoris so completely, that it was difficult to distinguish the meatus urinarius. Some of them were of considerable size—as large as a common fig—others were oblong, and were attached by a narrow pedicle. A great many small ones surrounded the orifice of the vagina. She also suffered from leucorrhœal discharge. No other symptom of importance appeared. The excrescences were the cause of a great deal of suffering and inconvenience, in a variety of ways.

Assisted by my friend Mr. Weston, I applied little wedge-shaped pieces of ice to the necks of the larger growths, till they became perfectly *blanched* and cold, and with a single stroke of a curved probe-pointed bistoury, removed several of the larger ones successively, without causing the patient any but slight pain.

I afterwards removed a very small growth without first applying ice. The result was, as might have been anticipated, the patient found the pain insupportable.

I found it necessary to introduce a bougie into the urethra, in order to indicate the position of the external orifice of that canal, so buried was it amongst the vegetations.

A great advantage obtained by the use of the ice, was the absence of hemorrhage; it being hardly requisite to apply a sponge during the operation. I was thereby enabled to proceed without hindrance, and a clear view was obtained of the exact extent of each sweep of the bistoury.

No reaction in the least degree excessive followed the proceeding, the progress of the case being satisfactory.

It appears to me, that by the above simple plan we may often save our patients considerable suffering, without exposing them to the least extra risk, which cannot be said of chloroform. It has been proved, on the one hand, over and over again, that if anæsthesia be not COMPLETE, there is the contingency of undesirable phenomena; while, on the other hand, if the anæsthesia be perfect, a greater chance of accident is incurred.—*Lancet, August* 31, 1850, *p.* 262.

139.—*Case of Vagitus Uterinus.* By A. S. Taylor, Esq., Lecturer on Chemistry and Medical Jurisprudence in Guy's Hospital.—Cases of vagitus uterinus are by no means common, and many alleged instances of this kind, reported in medico-legal works, are unworthy of credit. Any authentic or well-observed case is, therefore, acceptable. The following has been communicated to me by Dr. R. Crothers, of Moy, County Tyrone. In March, 1841, he observes, I was called upon to attend Mrs. W., in labour of her sixth child; her former labours had been quick. On my arrival I found her suffering from pretty strong pains; and on examination the membranes were tense, and protruding through the os uteri.

The labour not progressing so rapidly as I expected, I ruptured the membranes, found the face presenting, and apparently arrested at the the brim. I endeavoured to move it into a more favourable position, when happening to introduce a finger into the child's mouth, I was very much surprised to hear a *distinct cry*, which was repeated two or three times, and so loud, as to alarm the mother and attendants. The former was quite frightened, so that I had a good deal of trouble to compose her. I do not now remember if the child cried afterwards, without the fingers being introduced; but several times it did so, and so audibly, that I think the lungs must have been pretty well filled with air. I was obliged to complete the delivery by the forceps. The child is now a fine healthy boy.

Remarks.—The possibility of a child in utero, having its lungs so filled with air, as to be able to utter an audible cry, has been doubted. The above case, with a few others of a similar kind, reported in medical periodicals, removes all doubt on the subject.

In all the authentic instances that have been hitherto published, the uterine cry has been heard only where the face has presented, and the accoucheur has by any accident introduced his fingers into the mouth of the child. The air thus finds a passage to the lungs.

The only case in legal medicine on which the production of a uterine cry has any bearing, is in the application of the lung-tests in cases of infanticide. The air found in the lungs of a child may have been received into them before birth. Thus the fact that these organs floated on water, either entire, or when divided into small portions, would not furnish conclusive evidence, that the child had been born alive. It would merely prove the establishment of respiration, and not the fact of birth. These circumstances are, however, so well known to medical witnesses, that they scarcely require to be adverted to. The uterine cry has less interest, in a practical view, than vaginal respiration during the passage of the head; because in a case of unassisted labour, it is not easy to perceive how it could possibly occur; and if an accoucheur were present, then the case would not be likely to come before a medical practitioner as one of infanticide.—*Guy's Hospital Reports, Vol.* VII., *p.* 231.

140.—*On Indian Hemp as an Oxytoxic.* By Dr. Simpson, Edinburgh.—Dr. Simpson stated, that, in the early part of the winter session, he had given Indian hemp (Cannabis Indica) in several cases of tedious labour, with the view of ascertaining if it possessed any oxytoxic effect (like ergot of rye) in increasing and exciting the parturient action of the uterus. He had been induced to try the effects, if any, of Indian hemp during labour, in consequence of Dr. Churchill stating, that it possessed powers similar to those of ergot of rye in arresting hemorrhage, when dependent upon congested states of the *unimpregnated* uterus. In the few cases of labour in which it was tried, parturient action seemed to be very markedly and directly increased after the exhibition of the hemp; but far more extensive and careful experiments would be required, before a definite opinion could be arrived at relative to its possession of oxytoxic powers, and their amount.—*Monthly Journal, July,* 1850, *p.* 91.

141.—ON THE USE OF THE BOFAREIRA,

("RICINUS COMMUNIS" OF BOTANISTS) AS A MEANS ADOPTED BY THE NATIVES OF THE CAPE DE VERD ISLANDS TO EXCITE LACTATION.

By Dr. J. O. M'WILLIAM, F.R.S., R.N., &c.

While engaged in an official investigation into the nature and history of a yellow fever epidemy, prevailing in the Island of Boa Vista, in the Cape de Verds, during the year 1846, my attention was called to a remedy commonly had recourse to there, and in the other islands of the group, to accelerate and increase the flow of milk from the breasts of childbearing women, in cases where that secretion was tardy in appearing, or deficient in quantity when it did appear.

I also learnt that, on occasions of emergency, this remedy could be successfully applied to a still more important use, namely, to produce milk in the breasts of women who are not childbearing, or who even have not given birth to, or suckled a child for many years.

The leaves of a plant, called, in the language of the country, Bofareira, but which, in reality is the "Ricinus Communis" of botanists, and, occasionally, the leaves of the "Jatropha curcas," both belonging to the natural family *euphorbiaciæ,* are the means by which these interesting if not extraordinary results are produced.

The Bofareira grows in most if not all, the Cape de Verd Islands. That used by the natives for the purposes I have mentioned, is called by them the *white* bofareira, to distinguish it from what appears to be nothing more than a variety of the same species, the *red* bofareira. The *white,* or that which possesses galactagogue qualities, is recognised by the natives by the light green colour of the stem of the leaf, whilst the leaf stem of the *red* is of a purplish red hue. The latter plant is carefully avoided, as it is said to be a powerful irritant, and, if applied, as it occcasionally has been, by mistake, for the *white,* it produces an immediate and often immoderate flow of the menses..

In cases of childbirth, when the appearance of the milk is delayed (a circumstance of not unfrequent occurrence in those islands) a decoction is made by boiling well a handful of the *white* Bofareira in six or eight pints of spring water. The breasts are bathed with this decoction for fifteen or twenty minutes. Part of the boiled leaves are then thinly spread over the breasts, and allowed to remain until all moisture has been removed from them by evaporation, and probably, in some measure, by absorption. This operation of fomenting with the decoction and applying the leaves, is repeated at short intervals until the milk flows upon suction by the child, which it usually does in the course of a few hours.

On occasions where milk is required to be produced in the breasts of women who have not given birth to or suckled a child for years, the mode of treatment adopted is as follows:—

Two or three handfuls of the leaves of the Ricinus are taken and treated as before. The decoction is poured while yet boiling, into a large vessel, over which the woman sits so as to receive the vapour over her thighs and generative organs, cloths being carefully tucked around her so as to prevent the escape of the steam. In this position she remains for ten or twelve minutes, or until the decoction cooling a little, she is enabled to bathe the parts with it, which she does for fifteen or twenty minutes more. The breasts are then similarly bathed, and gently rubbed with the hands; and the leaves are afterwards applied to them in the manner already described. These several operations are repeated three times during the first day. On the second day, the woman has her breasts bathed, the leaves applied, and the rubbing repeated three or four times. On the third day, the sitting over the steam, the rubbing, and the application of the leaves to, with the fomentation of, the breasts, are again had recourse to. A child is now put to the nipple, and, in the majority of instances, it finds an abundant supply of milk.

In the event of milk not being secreted on the third day, the same treatment is continued for another day, and if then there still be want of success, the case is abandoned, as the person is supposed not to be susceptible to the influence of the Bofareira.

Women with well-developed breasts are most easily affected by the Bofareira. When the breasts are small and shrivelled, the plant then is said to act more upon the uterine system, bringing on the menses, if their period be distant, or causing their immoderate flow if their advent be near.

Exposure to cold is carefully avoided by persons who are being brought under the influence of the Bofareira. They scrupulously abstain from wetting with cold water either the hands or the feet.

Maria, a dark mulatto woman, with woolly hair, thirty years of age, tall, stout, and well-formed; menstruating regularly; the mother of three children, the youngest of whom was three years old, and had been weaned when under the age of one year, was brought before me by Dr. Almeida, of Boa Vista, on the morning of the 30th of June, 1846, for the purpose of being submitted to the action of the Bofareira. She stated that when her child was weaned, every trace of milk disappeared from her breasts in the course of a few days. I could not detect any sign of pregnancy. The breasts were like those of negro women in general who have borne children, pendulous and flabby. No sign of milk was given out from them upon careful expression of the nipple.

The baths, fomentations, the application of the leaves, friction, suction, &c., were adopted in the manner and order I have already described. On the second day there was a slight oozing of a serous-looking milk from the nipples, with slight increase of size in the areolar portion of the breast. On the third day the milk was increased in quantity, and less watery. On the morning of the fourth day there was evident enlargement of the lower part of mamma, and milk flowed abundantly upon the application of a child to the nipple.

The use of the Bofareira in cases of childbirth, to accelerate the flow of milk, is common, but comparatively rare as a means of procuring a wet-nurse. Some instances of the latter kind occurred, in consequence of the death of mothers with children at the breast during the progress of the Boa Vista epidemy of 1845-46, which decimated a population consisting almost wholly of blacks, with a few Europeans (Portuguese and English) and a small proportion of mixed negro and European blood.

Generally, however, this use of the Bofareira is seldom called for. Death in childbirth, or prolonged illness after parturition, sometimes requires a kind relative or charitable neighbour, who, for the safety of the offspring, places herself under the influence of the Bofareira.

The son of a wealthy landed proprietor of San Nicolao (well known to my friend, Mr. George Miller, of that island), a remarkably hale and robust looking man, was wet-nursed by a woman who gave him milk produced by the bofariera. The nurse in this instance had borne two children in early life. Her husband died shortly after the birth of her second child; she lived in a state of virtuous widowhood, and it was *many* years after the death of her husband that she so generously submitted herself to the bofareira, and nursed the infant in question.

Consul-General Rendall, of the Cape de Verds, informs me that a lady, a native of Boa Vista, now residing at San Antonio, and the wife of one of the foreign consuls, had a daughter in 1843. "Having very little milk," says Mr. Consul Rendall, "she caused an old female servant to be prepared with the bofareira, and to act as wet-nurse, which she did in the most satisfactory manner, having plenty of good milk, although she had not had a child for ten years previously. The child is now (March, 1847) a healthy one, and well-grown. In short," continues M. Rendall, "women who use the bofareira are in two or three days in order sufficient to nurse the child of a queen."

I have not been able to ascertain, from personal observation, or from any very accurate information, what effect the bofareira has upon virgins, or upon those who, although they have not borne children, are nevertheless not virgins. As regards the latter class, however, an intelligent native midwife assured my most able and observant friend, Mr. George Miller, of San Nicolao, that the effect of the administration of the bofareira is much the same upon them as upon childbearing women.

In some cases, but rarely, the decoction of the bofareira is taken internally, with a view of assisting the action of its external application.

I regret not having been informed of the alleged difference in the action of the white and red bofarieras, while I was at the Cape de Verds, that I might have examined the latter plant upon the spot.

The seeds of each plant were, however, kindly forwarded to me by Mr. George Miller, and Sir William Hooker most readily and obligingly examined them. Sir William, in a note to me, says, "What you mark as red bofareira and as white bofareira, are both, not only of the genus 'recinus,' but also of one and the same species—viz., ricinis communis, the common palma Christi, or castor-oil plant. In our gardens, as well as abroad, the plants vary, and your two plants vary a little in the form and size of the seed, and especially in the colour, but they are one and the same species."

It is thus evident that the white and red bofareiras, if they differ at all, can only be varieties of the same species. It is known, however, that certain varieties of other plants, as thyme, mint, &c., do yield different properties, and such may be the case with the bofareiras.

I have thus stated all the facts that have come to my knowledge regarding this galactagogue of the Cape de Verds, which I consider to be well worthy of a fair trial in this country. Should its action in our more temperate regions be similar to that which it exerts within the tropics, an interesting field of enquiry will be opened, as regards its hygienic, medical, medico-legal, and other relations.

These, however, are points, the consideration of which had better be reserved until it has been determined, by experiment, how far the bofareira can be successfully introduced into the practice of this country.

Note.—Dr. Tyler Smith, to whom I showed my paper before my visit to Edinburgh, has written to inform me that he has in several cases tried the bofareira in the manner described by me; and he assures me that the effects of the plant grown in this country fully bear out the facts I have detailed respecting the use of this plant in the Cape de Verd Islands.—*Lancet, Sept.* 7, 1850, *p.* 294.

142.—*On the Galactagogue and Emmenagogue Effects of the Leaves of the Bofareira (Ricinus Communis, or Palma Christi).* By Dr. Tyler Smith.—[Dr. Smith states, that being struck by the facts related in Dr. M'William's paper, and learning for the first time that the plant is no other than the ricinus communis, which grows as an annual plant in this country, he determined to ascertain by Dr. M'William's wish whether the plant when growing in our latitudes, preserves its remarkable properties of stimulating the mammary glands. Dr. Smith says:]

In directing the use of the Bofareira leaves, which I have procured from the Botanical gardens at Chelsea, Kew, and the Regent's Park, I have followed as nearly as possible the description of Dr. M'William, with the exception of the application of the steam of the decoction to the generative organs. The following are the cases in which the agent has been used under my directions.

The following case was conducted by Mr. C. Stillman, one of the house-surgeons of Queen Adelaide's Lying-in-Hospital.

Case I.—Mrs. C., twenty-four years of age, rather tall and thin, mother of two children, had weaned the last about six weeks, and had still a little milk, of a very thin serous character, left in the breasts. She commenced the use of the Bofareira, on the morning of Wednesday, August 21st, by bathing the left breast only, with a strong decoction of the leaves. The leaves themselves were afterwards applied to this breast. In the evening, she repeated the bathing; after which, she perceived, on squeezing the nipple, that her milk, which was at first thin and watery, had now become quite thick. After repeating the application on Thursday the 22nd, she felt throbbing pains in the breast, accompanied by sickness and pains in the back, which she described as being like after-pains, and the areola surrounding the left nipple had become much darker than the right; the glandular follicles were also larger than in the nipple which had not been under the influence of the Bofareira. The difference between the two breasts was very marked. Having at this time no more leaves, she was unable to continue the application. On the following day, a fresh supply of the leaves was obtained, and she again bathed her left breast as before. After two applications, the *catamenia* appeared before the regular time, and the fomentations were not afterwards continued.

Case II.—Mrs. H., mother of four children, her youngest child aged one year and five months, had been weaned more than six months. During the latter months of lactation, she had little milk; the breasts were small, and the nipples contracted. Before applying the Bofareira, the breasts were carefully examined, to learn if they still contained any traces of milk. After much trial, she could squeeze from her left breast the smallest points of serum from the mouths of two or three of the galactophorous ducts, as is the case with most women who have suckled; but from the right breast not a trace of moisture could be expressed. The Bofareira was used night and morning for four days, by bathing with the decoction, and the application of the hot leaves to both breasts, in the manner described by Dr. M'William. After the second application, thick milk, like the colostrum, could be squeezed from both nipples, the breasts were considerably swollen, the glands in the axillæ were also painful, and pains extended down the arms. There were, in fact, in this case, all the symptoms present, in a minor degree, which are usually observed in the establishment of the milk after parturition. Mrs. H. had also distinct periodical pains in the back and abdomen, which she compared to after-pains. A leucorrhœal discharge was also produced. At the end of the fourth day, milk flowed so freely into a breast-pump, that there was no doubt she could have suckled a child; but at this point the application of the Bofareira was omitted, and the milk has since gradually disappeared.

Case III.—Mrs. D., a married lady, without family, hearing of the use of the Bofareira in the last case, wished it tried upon herself. As there was no possibility of injury she was supplied with some of the leaves, and proceeded to use it. The application, and the use of the decoction, produced swelling of the breasts, pain in the back, and an increase of a leucorrhœal discharge, to which she is subject; but there was no appearance of milk in the breasts. At the time of using the Bofareira, the catamenial discharge had ceased about a week.

Case IV.—M. L., a young woman, who had been delivered three weeks, but whose milk, though profuse, was so poor as to be little more than serum, used the Bofareira three times. Under its use the secretion from the breasts became markedly thicker; but the child was unfortunately attacked with diarrhœa, and it was not thought advisable to continue the use of this agent longer.

Case V.—L. M., a young woman, mother of one child, but who had weaned her infant about a year and a half, applied the Bofareira in the form of decoction and poultices two or three times; but the pain and swelling were so considerable, that she refused to go on with it. She had a little serum in the breasts, at the time when the use of the Bofareira was commenced. The secretion speedily became milky. This patient had a leucorrhœa, which had been present ever since the weaning; and the uterine and vaginal irritation, upon which the leucorrhœa depended, had kept up, in all probability, the serous secretion from the breasts, which is common enough in leucorrhœal cases.

Since the foregoing cases occurred, I have used the remedy in a case of scanty menstruation of a remarkable kind. Owing to exposure to marsh malaria, some years ago, the patient had scarcely a sign of coloured discharge at the usual catamenial periods. She used the infusion of the leaves of the red Bofareira at the date of her period, applying the infusion and leaves to the breasts, and the vapour to the genitals, with the effect of producing, in two days, a considerable flow of the catamenia. From the effects in this case, and in one of the cases already related, the Bofareira promises to be of considerable value as a direct emmenagogue; at all events the cases in which I have tried it, show that the plant does not lose its efficacy in this climate. I hope that, in America and other parts in which the plant is common, perennial instead of annual, extensive trials of its efficacy both as an emmenagogue and a galactagogue, will be made. —*Lond. Journal of Med., Oct.* 1850, *p.* 951.

143.—*On a Japanese Remedy for Sterility.* By Dr. E. WILLIAMS.—[Dr. Williams believes there is a botanical remedy in Japan and China which exerts a specific influence upon the uterus, more particularly in cases of sterility and checked menstruation. The tree is one of the order Ternstromaceæ of Jussieu, growing to the size of the English laurel, with leaves somewhat larger than the congou tea, and the odour resembling pulegium and sabina.]

The mode of preparation is to take a quantity of the leaves, macerate them in as much rice-spirit (samshu) as will just moisten them for six hours; then express and give about a teaspoonful every hour, and two or three doses will invariably bring on the menstrual secretion, which can be maintained by a dose or two daily for any length of time.

Females in the healthy state are expected to menstruate in their eighth year in Japan and China, and should they not do so they are ineligible for betrothal; therefore recourse is had to the *key tu sing* with certain results.

To ensure its success, according to popular belief, the leaves must be gathered by a virgin the while using certain cabalistic formulæ, at the full of the moon, and during the burning of a certain number of highly perfumed jop-sticks.

When required for the purpose of obviating sterility, the tree must be in its second year, and also be gathered with certain prescribed formulæ at the wane of the moon, and equal parts of the root must be added to the preparation, which is made in the same manner as the preceding receipt.

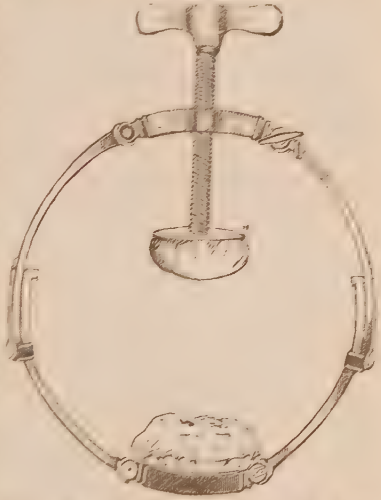
That the root is aphrodisiac in its effects I have not the slightest doubt, as I have administered it to animals with obvious results, and without any ill effects even to mules and castrati.—*Lancet, September* 14, 1850, *p.* 321.

ADDENDA.

144. AMPUTATION FOR CARCINOMA, AND DISEASE OF THE KNEE-JOINT. MR. SKEY'S NEW TOURNIQUET.

Under the care of Mr. LAWRENCE.

[Mr. Lawrence used a new tourniquet in operating upon this case: an instrument invented by Mr. Skey, and manufactured by Mr. Fergusson. The following is a sketch of the same:]



It will be noticed that this tourniquet is intended to encircle the leg or thigh completely; it requires no piece of roller or bandages of any kind, and is, so far as we have seen, adapted with the greatest ease. The two elliptical halves are made of steel, about a quarter of an inch thick; they are connected above with the plate holding the screw, by two hinges, one on either side of the plate; and likewise connected with the plate which supports the cushion below, by two other hinges on either side of the latter. All stiffness is thus thoroughly avoided, and the circular figure formed by the whole instrument may be altered in various ways, and adapt itself to the shape of the limb. The hinge on the right side of the screw is provided with a peg, which, by being withdrawn, allows the branch to be completely separated from the plate, and the instrument to be opened for the purpose of applying it or taking it off. It will be perceived at one glance that the cushion below is intended for the posterior part of the limb; and when the pad attached to the screw is directed downwards, and applied to the artery, the thigh is not tightly surrounded and the return of the venous blood arrested, but the pressure on the artery is effected merely between the pad above and the cushion below. On examining the centre of the rods, it will be seen that they are composed of two equal parts, the upper receiving the lower in a ring, by which contrivance they may slide upon one another, and the instrument be rendered more or less capacious, to meet the necessities of any size. The sliding parts are provided with holes and a peg, by which the form of the tourniquet may immediately be fixed.

The preceding cut shows that the lower portion of the rod may slide upwards, and render the instrument of a smaller size. Mr. Skey applied this tourniquet himself, in the case above mentioned, and it evidently answered the purpose in a very satisfactory manner, all the surgical staff present being unanimous in the expression of their approval.—*Lancet, October* 12, 1850, *p.* 420.

145.—*Treatment of Nævi by Compression, or Strangulation by a peculiar Knot.—Cutaneous Cancer of the Hand; Amputation.* By Mr. Fergusson.—[When Mr. Fergusson wishes to destroy these morbid growths by strangulation, he does not follow the usual method of transfixing the nævus with two pins at right angles with each other, and twisting strong silk round them, but proceeds in the following manner:]

He armed a common curved surgical needle with a strong thread, about eighteen inches long, and passed the needle (the thread being double) through the base of the tumour. One of the threads, about two inches from the eye of the needle, was then cut across, and Mr. Fergusson introduced one of the loose ends, lying on the other side of the tumour and corresponding with the cut thread, into the eye of the needle, which latter then carried two loops. (The end to be chosen is the one continuous with the thread first cut.) The needle was now carried at right angles to the direction it took at first, and when it was disengaged from the threads, the latter presented around the tumour *two loops* and *four ends.* Mr. Fergusson now pulled the threads gently, so as to ascertain their relations, and he then made two tight knots with the four loose ends. Thus the tumour was effectually strangulated, being surrounded, not only with two knots, but with four firm *loops.*

[The treatment of nævi appears to require various modifications according to the size of the growth, and the locality whence its springs. In a case where the tumour was situated on the cheek, and where it was desirable to obtain obliteration without much loss of blood, or an unsightly cicatrix, Mr. Fergusson proceeded thus:]

Two strong pins were first thrust through the base of the tumour (which was about the size of a crown piece) from below upwards, at about one inch distant from one another; and two others were passed transversely, being separated by the same space as the first. Mr. Fergusson then rolled a narrow strip of lint into the shape of a thick string, and applied it firmly round the circumference of the swelling, and under the pins. This constriction having thus steadied the tumour, a circular pledget of lint, about a quarter of an inch thick, was then placed upon the nævus, and secured in that situation by strong silk threads, which Mr. Fergusson wound round the pins, and carried across the pledget. The latter, by these means, exercised a powerful pressure upon the growth, which was expected to be sufficient to cause the complete atrophy of the latter. Nor was Mr. Fergusson deceived in his expectations. Some weeks afterwards when the needles had been withdrawn, and some ulceration which had taken place at the orifices, had cicatrized, the tumour seemed all but obliterated.

[Mr. Fergusson states that in this manner pressure of a very effectual kind may be applied to any part of the body with a great amount of certainty. He had used it in many cases for the last fifteen years, and had every reason to be satisfied with it, though it was not applicable in all instances. He in general prefers tying such tumours, but even the ligature, however well applied, sometimes fails.]—*Lancet Oct.* 12, 1850, *p.* 421.

146.—*Hare-Lip; Direction given to the Knife in the Paring of the Edges* —A few weeks ago, Mr. Fergusson had to rectify, upon a child, about one year old, the deformity of hare-lip. The operation was performed in the usual manner, with the exception of the direction given to the knife whilst the margins of the fissures were being pared. As the scalpel run straight from above downwards, Mr. Fergusson made it cut slightly inwards on both sides, when he reached the red portion of the lip, so as to save a little more of that portion than of the part covered by skin. This precaution is likely to remedy a defect which is generally left after the union of parts in hare-lip. Every surgeon has noticed the peculiar notch which the red part of the lip presents after the linear wound above it has completely united; this is more or less the case with various patients, and it must have struck many practitioners that it would be very desirable to obviate this defect, and bring about the complete union of that part of the lip which is immediately continuous with the mucous membrane of the mouth. The angle inwards, with which Mr. Fergusson modified the usual rectilinear paring of the edges, is likely to effect this desideratum, and will tend to lessen very considerably the deformity left after the operation for hare-lip. Mr. Fergusson has been in the habit of modifying the incision in this manner for the last seven years, and was sorry that this method, which he believed was of French origin, was not more followed in this country. This plan was recommended by Professor Miller, of Edinburgh, and, as he (Mr. Fergusson) had learned from a recent conversation with Messrs. Smith and Teale of Leeds, was followed by some of the surgeons in that town. The child has progressed very favourably, complete union has taken place, and no notch is perceptible on the lip.—*Lancet*, *Oct.* 12, 1850, *p.* 421.

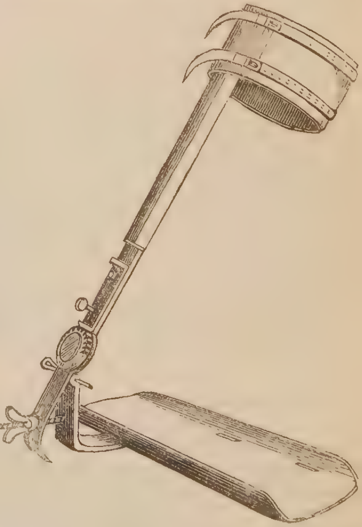
147.—TALIPES VARUS —NEW FORM OF APPARATUS FOR EXTENSION AFTER OPERATION.

Under the care of Mr. LE GROS CLARK.

[The patient was a little girl, upon whom Mr. Clark had divided the tendo achillis, to remedy congenital talipes varus.]

The patient, Mary R., aged fifteen, was admitted into St. Thomas's Hospital, under the care of Mr. Le Gros Clark, in November, 1849. The case presented an illustration of that extreme form of talipes varus, in which there was not only entire inversion of the foot, but where it was also turned over, so that the plantar region was directed upwards, whilst the dorsal surface of the tarsus rested on the ground (talipes dorsalis of some authors). A large bursal pad was developed in the latter position, and progression performed with great awkwardness and difficulty. The whole limb was shrunk, especially below the knee. and the foot shortened and curved upon itself, the toes overlapping each other. The deformity was congenital.

Shortly after the patient's admission, Mr. Clark divided the tendo-Achillis in the usual position and manner, and the foot was put up loosely in Stromeyer's apparatus. The inconvenience and imperfection of the latter became very conspicuous in this case at a very early period, for it occasioned considerable pain up the leg, and vesications on the toes, dorsal surface of the tarsus, and heel. This induced Mr. Clark, who had felt dissatisfied with all the mechanical forms of treatment which had come under his observation, to consult with the instrument-maker to the hospital, in order that an apparatus might be constructed, in itself more simple, efficacious, and easy of management, and, at the same time, less irksome to the patient. The following description of the instrument, which was immediately manufactured for, and first used in this case, was supplied by Mr. Milliken, Mr. Bigg's intelligent foreman in St. Thomas's-street, who superintended its construction.



"The instrument is intended for talipes equinus, valgus and varus, and consists of a leg-piece, with circular strap, to pass round the leg under the knee, an ankle-joint, forked extremity, and foot-piece. The joint at the ankle allows of the foot being placed at any angle, the upper part of this joint is cut in cogs, and a sliding catch is attached to the leg-piece, which fits into the cogs, and secures the foot when the toe is raised. Immediately below the joint is a hinge, which allows of the foot-piece being detached at pleasure. The under part is attached to the latter, and is furnished with a nut and screw, playing in the forked extremity. The foot-piece has a ridge at each side to confine the foot, and five long holes cut in it—viz., one at the heel, and two at each side, in which the strapping is to be secured. Detach the foot-piece, and strap the foot down to it, in as straight a position as possible, and over the strapping (adhesive plaster) place a roller, leaving the portion for the hinge free. Now apply the leg-piece, and put the hinge together, letting the foot take its own position. Gently twist the affected foot with the hand, but not so as to give pain, and screw the nut so as to keep it firm. Then raise the toe of the instrument in the same way, and secure it by the latch. Examine the strapping, and snip it with a pair of scissors where it would be liable to cut. The instrument should be attended to daily."

This instrument was prepared and ready for application, on the fourth day after the operation. Before applying it, a broad and strong band of the plantar fascia was freely divided, by which the antero-posterior curvature of the foot was at once relieved, so that it could be placed flat on the foot-piece of the new apparatus.

The instrument was worn, without inconvenience to the patient, for a lengthened period; and the blisters on the toes, heel, and tarsus speedily healed. It was necessary once to intermit its use for a short time, in consequence of the patient suffering from chilblains in very cold weather. The screws were almost daily slackened from an early period, to allow of the employment of passive motion; and thus some ground was gained in effecting the requisite extension of the ankle joint.

When the apparatus was left off, a boot, with an iron support, extending up the leg, was substituted. The patient was soon able to walk, but everted the foot, whilst the knee was inverted; and though the foot was planted quite flat on the ground, it was *thrown out*, at each step, just as paralytics walk. In fact, it was evident that the extensors of the toes and flexors of the ankle were quite powerless; and no motor influence had been regained over them when she left the Hospital, early in June, though the position and shape of the foot were natural.

Mr. Le Gros Clark remarked, respecting this case, that it offered an interesting illustration of the remediability of an extreme form of distortion, by the application of a suitable apparatus after operation. But he attached much importance to the employment of daily passive motion, after the expiration of, at most, the first week; at the same time that he deprecated continuous extension, which was painful to the patient. Indeed passive motion is much more effective in procuring the same result, with infinitely less risk and suffering to the patient. He observed that it must be a subject of speculation, whether the paralyzed condition of the tibialis anticus and extensors of the toes was itself the cause of the deformity, or a consequence (from continued extension) of the retroversion of the foot. Time will prove this, as the latter defect will probably be eventually remedied, in part, if not entirely; but if this be defect of original nervous organization, it will be permanent. *Lancet*, *September* 7, 1850, *p.* 303.

148.—REMARKS ON THE ADMINISTRATION OF CHLOROFORM, IN ANSWER TO PROFESSOR LIZARS.

By Dr. JOHN SNOW.

[Upon the respective merits of the inhaler and handkerchief, in the administration of chloroform, Dr. Snow remarks:]

To prove that the handkerchief, and not the use of inhalers, has been the chief cause of the deaths from chloroform, it is only necessary to enumerate the fatal cases.

1. Jan. 28, 1848. Hannah Greener, aged 15, near Newcastle. Toe-nail operation. *Handkerchief.*
2. Feb. 23, 1848. Mrs. Simmons, of Cincinnati, U.S. Extraction of teeth. *Inhaler; no medical man present.*
3. A young woman at Hyderabad, in Hindostan. Distal phalanx of a finger. *Handkerchief.*
4. May, 1848. Madlle. Stock, aged 30, at Boulogne. Opening an abscess. *Handkerchief.*
5. Dec., 1848. Young gentleman, at Govan, near Glasgow. Intended toe-nail operation. *Handkerchief.*
6. Jan. 24, 1849. J. Verrier, aged 17, Lyons. Intended amputation of finger. *Handkerchief.*
7. Feb. 20, 1849. Samuel Bennett, Westminster. Amputation of toe. *Handkerchief.*
8. Aug. 23, 1849. Mdme. Labrune, Langres, France. Intended extraction of tooth. *Handkerchief.*
9. Oct. 10, 1849. John Shorter, aged 48, St. Thomas's Hospital. Toe-nail operation. *Inhaler; given by a non-medical person.*
10. Nov. 1849. Girl Jones, Shrewsbury. Removal of eyeball. *Not stated.*
11. Young lady, Berlin. Intended extraction of tooth. *Napkin.*
12. Feb., 1850. Artilleryman, Mauritius. Amputation of last phalanx of middle finger. *Handkerchief.*
13. Alex. Scott, aged 34, Guy's Hospital. Removal of portion of hand. *Handkerchief.*

These, I believe, comprise all the recorded cases in which death was clearly due to the administration of chloroform.

In one of the cases it is not stated that a handkerchief was used, though, from the context of the report, that seemed to be the case. Of the other twelve an inhaler was used in only two of the cases, and in neither of them was it used by a medical man, whilst, in the ten instances in which death was caused by chloroform on a handkerchief, it was administered by a qualified medical man in every instance but one. There have been four cases in which death was accidentally caused by chloroform taken by the diseased parties when no one else was present. Two of these deaths occurred in Scotland. A handkerchief was used in each instance, and, but for the practice of taking chloroform on a handkerchief, these persons would probably never have taken it all. However, I have not included these cases in the above list. There was a death commonly attributed to chloroform which I have not included in the list,—that which occurred at Mr. Robinson's, the dentist; for I believe that that was a case of fatal syncope, due to mental emotion in a subject with fatty degeneration of the heart, and great enlargement of the liver. He was only just beginning to inhale, when he suddenly died. In that case an inhaler was employed.

Other supposed causes of the deaths from chloroform have been suggested by various writers, besides those above mentioned, only one of which I shall notice. Many persons, especially in Edinburgh, have attributed the fatal cases to the impurity of the chloroform employed; but, in the first place, it has been examined in nearly all the cases, and found to be quite good. And, again: although it might contain impurities which would cause irritation at the time, and subsequent disagreeable effects, it could not become contaminated with anything likely to cause sudden death; and, before this assertion is repeated, we ought to be told with what substance stronger than itself chloroform is liable to be contaminated. I fully admit the necessity of having chloroform, as well as other medicines, quite pure; as its adulteration, though not liable to cause death, may cause disappointment, and other disagreeable consequences; and it must also be admitted, that this as well as other drugs, is liable to impurity, both in London and elsewhere. I am of opinion, however, that impure chloroform has not been supplied by the respectable houses of this metropolis. I generally use Mr. Morson's chloroform; but have, at one time or other, used that of Mr. Squire, Mr. Jacob Bell, Mr. Bullock, Mr. Hooper, Mr. Taylor, and others, and always found it good. I have also used the chloroform of Messrs. Duncan, Flockhart and Co., of Edinburgh, and cannot give it any better praise than by saying it was equally good. If the Edinburgh people wish to assert that they can produce something better than pure chloroform, they must call it by another name, for it must be a different medicine.

The real cause of the deaths from chloroform undoubtedly is, that in each case the patient has had an overdose; I mean more than was necessary to render the patient, or one of similar size and strength, insensible. By a dose of chloroform, however, must be understood, the quantity that is in the system at one time, and not the quantity inhaled during an operation. For instance, when the inhalation is left off for two or three minutes, a great part of the chloroform exhales by the breath, and the patient perhaps requires to inhale a little more. This should be considered a repetition, and not an increase of the dose.

The necessary points to be observed, in order to avoid the risk of giving an overdose, are, 1st, that its vapour be systematically diluted with a sufficient quantity of air, by means of a suitable apparatus—when no accident can happen without the continued neglect of evident warning symptoms; and, 2nd, that the person administering the chloroform should keep his whole attention directed to the patient, and be able to understand all the signs that occur. I have not space here to go fully into the description of the signs which indicate when the inhalation should cease, and an operation begin, but would rather refer to some papers of mine, 'On Narcotism by Inhalation,' in the 'Medical Gazette,' 2nd vol. for 1848, and also to some papers on 'Chloroform,' by Dr. Sibson, in the same volume. I may state, however, that it is chiefly by attention to the state of the respiration and the eye, that danger is to be avoided. The pulse may be felt as a physiological inquiry, or with reference to the operation, but gives no guiding information concerning the chloroform, for the following reasons:—When the vapour is diluted to a safe extent, it might be continued till death, as I have ascertained in animals, and the pulse would still beat distinctly for many seconds after the respiration had ceased; and if, on the other hand, the vapour be of dangerous strength, the heart might suddenly cease to beat, and the first intimation of danger from the pulse would come only too late.

If chloroform be given on a handkerchief at all, not more than from fifteen to twenty *minims** should be put on at once. In this way it may be given in midwifery, as that quantity, and often much less, suffices for each application; but in surgery, where the full effect is required, this would be insufficient.—*Medical Times, Aug.* 31, *p.* 230.

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149. *Danger of Chloroform.*—[The following is the opinion of Mr. Cock on the administration of chloroform, which was given at an inquest held upon Alexander Scott, who died from the effects of the drug.]

Mr. Cock, the operator, said, at the inquest, that in many cases where chloroform had been used, he had waited with breathless anxiety for the recovery of the patient from its influence. The public ought to know the great danger of using it, and he strongly objected to its administration under any circumstances.—*Medical Times, July* 6, 1850, *p.* 25.

150.—*On the Use of Chloroform.* By Dr. Charles Kidd.—[On referring to the case of the death by chloroform, under Mr. Cock, Dr. Kidd asks, What is the real amount of danger attending the use of this agent? He remarks:]

As to its effects on the system, we did not require M. Bouisson to tell us the exact order in which the organic and vital functions become affected by chloroform; they are evident at a glance. A more practical fact was lately stated in Paris, viz.:—that oxygen is a perfect antidote to these deadly influences,—a point worth recollecting, as bearing the semblance of truth. We have then the astounding fact stated by Gregory, of Edinburgh, that what is sold as chloroform is not chloroform at all: some specimens he examined contained only 1-20th of the true anæsthetic principle! The best test suggested, is agitating the chloroform with sulphuric acid, and afterwards with peroxide of manganese; if it blackens, it is impure. Specific gravity is another test: if not once-and-half heavier than water, chloroform is not pure. By keeping, I fear, chloroform does not improve; at least it should be in well-filled and well-stoppered bottles. All specimens, after a little, show unmistakeable signs of hydrochloric acid, and even free chlorine; such are unfit for use. Pure chloroform should have little or no odour; the latter, though so pleasant and fruity, being anything but a recommendation. We are here, then, simply between the Scylla and Charybdis of these chemical perplexities, on the one hand; and the chances of obscure organic disease of the central parts of the nervous system, on the other,—both of which require the utmost circumspection on the part of the surgeon. The chloroform used at Guy's is as pure as any in London, and has been exhibited over and over again without ill effects, so that in the case under notice we ought perhaps to look for something wrong in the spinal and ganglionic systems.—*Medical Times, July* 20, 1850, *p.* 63.

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* Drops of Chloroform are widely different from minims, being extremely small.

151.—*On Poisoning by Strychnia; with Remarks on Experimental Enquiries into its Effects on Animals.* By GEORGE BENNETT, Esq., M.R.C.S., Sydney. During a visit to Manilla, I tried experiments of the poisonous effects of the seeds of the *ignatia amara* (named *cabalonga* by the natives) in two instances; one on a dog about ten months old, and with the following results:—At fifty-five minutes past ten A.M., I gave half a drachm of the seed, cut into small pieces and enveloped in a piece of meat, to a dog: it was swallowed without any of the pieces being lost. The dog was lively and playful, but at times would appear restless and uneasy, as if some internal annoyance was occasioned by the poison. At twenty minutes past eleven he suddenly fell, with violent convulsions of the limbs, which were extended with great rigidity, and they afterwards remained in that rigid position; this was followed by excessive panting and trembling of the muscles; the saliva became viscid, and the tongue, as well as the saliva with which it was covered, had a dark appearance. The poison acted on the nervous system; the dog exhibited no indication of pain; no yell escaped him; the eyes assumed a dull appearance; the mouth had a movement as if the animal had been snapping at flies; and there was a constant spasmodic twitching of the muscles of the face. At twenty-five minutes past eleven, the body became less convulsed; the urine was passed involuntarily, but not the fæces. On passing the hand before the eyes of the animal, the eyelids were moved, indicating that consciousness had not yet departed; the orbicularis oculi muscles had a convulsive quivering; the snapping and panting still continued. At thirty-five minutes past eleven, a general convulsive action of the muscles came on, and terminated the existence of the animal.

An hour after death I inspected the body. On opening the thorax, the viscera had no unusual appearance. On inspecting the stomach, it was found partly filled with rice, among which the small pieces of the seeds of the *ignatia amara* were intermingled: on emptying the stomach of its contents, the inner coat was found of a pale, pinkish hue. The liver had a healthy appearance, but if any portion of that organ was cut, a quantity of blood, in a fluid state, immediately flowed; (and it is an interesting question, why we find the blood destitute of fibrin in those cases in which death proceeds from causes acting immediately on the nervous system, as where a person is struck dead by lightning, &c.?) The remainder of the abdominal and pelvic viscera had a healthy appearance. The whole of the muscles of the animal had a bloodless, blanched appearance; the vena cava and aorta were also filled with blood in a fluid state. On inspecting the brain, no particular appearance was observed, excepting that the vessels appeared totally destitute of blood.

The second experiment was on a dog, a year and a half old. At thirty-five minutes past four P.M., half a drachm of the seed, cut into small pieces, was given to the animal. At five minutes past five P.M., he suddenly fell with violent spasms of the limbs, which soon became stretched out and rigid, with spasms of the lower jaw. I was desirous of trying vinegar as an antidote; so, as soon as the spasms of the muscles of the lower jaw relaxed, I poured down some quantity; the animal appeared nearly dead. About two minutes after the vinegar had been administered, he was so much recovered as to stand up: it was, however, but of short duration, for at ten minutes past five he reeled and fell, with similar spasms as before. Another dose of vinegar was given, with the same good effects: the animal recovered, and stood up; the eyes, however, preserved throughout a dull, glairy appearance. The dog appeared uneasy, as might have been expected, and trembled exceedingly, but no expression of pain escaped from him; he seemed bewildered. At fourteen minutes past five he fell, with similar effects as before; the spasms were not, however, so severe. The same remedy was given, with the same reviving effects. The fits were accompanied at first, by violent spasmodic action of the muscles of the lower extremities, which afterwards became extended and rigid, whilst other parts of the body were convulsed. The muscles of the jaw had invariably a strong spasmodic action at the commencement of the fit, but soon became relaxed. The panting and snapping were not so marked in this as in the preceding experiment. At eighteen minutes past five he again fell; the urine was passed involuntarily, but not the fæces, and he appeared nearly dead. Vinegar was again administered; but he lay for some time without hardly an indication of existence. I thought life was extinct. At twenty-five minutes past five, the respiration, before scarcely to be perceived, became laborious, as if the circulation of the blood had again taken place, by a renewal of the heart's action; the eyes became animated, and the dog regained his sensibility; the laborious state of respiration only continued for a short period. At thirty-five minutes past five, he had not risen from his extended position on the ground, but no fit had again yet taken place. On causing him to be raised from the ground, the limbs displayed a contracted, rigid appearance, but they could be readily placed by the hand in a relaxed position. He was soon afterwards able to sit up by his own exertions; he was then so far recovered as to run the length of the cord to which he was tied, but the limbs, when he moved, appeared stiffened and paralytic. The animal, a short time previous to his being brought to me, had received an injury of the left hind leg, which had occasioned lameness: when the spasmodic action of the muscles came on, during the paroxysms the injured limb was drawn upwards and backwards, whilst the other limbs were drawn, stiffened downwards and outwards.

The animal continued apparently well, but in a miserable, debilitated state, until fifty minutes past five, when he moved about, and fell down suddenly, with the same tetanic symptoms as before; the eyes had again their dull, glairy appearance; the jaws were kept firmly closed by the violence of the spasms, which severe spasms were extended over the whole of the body; and in the space of a minute he was dead. The quantity of vinegar taken was about half a pint.

The poisonous effects of these seeds are speedy and violent. Magendie considers that its active principle—the strychnine—strongly excites the spinal marrow, without affecting, except indirectly, the functions of the brain. After the dog has taken the poison, his playful manner still continues, but mixed, at the same time, with a restlessness, which increases as the poison begins to act; he then feels inclined to lie down, then rises again, until he suddenly falls with tetanic symptoms, and during the action of the poison the animal appears bewildered.

The active principles of the *ignatia amara* are strychnine and brucine, of which the first is the more powerful, and is said to be found purer in the ignatia than in the nux vomica; but it is met with purest in the upas tienté of Java.—*Lancet, Aug.* 31, 1850, *p.* 259.

152. *Chloroform in a Case of Poisoning by Strychnine.*—By Dr. MUNSON.—Mr. G., aged about 40, of intemperate habits, took from among some medicines, on the 5th instant, a bottle of strychnine; and supposing it to be morphine, as he said, swallowed a dose supposed to be about one or two grains. In about twenty minutes afterwards Dr. Munson was requested to see him, as he was supposed to be in a "fit." He found him in the following condition:—The whole muscular system rigid; the muscles of the back, and of the upper and lower extremities, rigidly contracted; the head drawn back; articulation difficult; sense of tightness about the chest, perspiration flowing profusely from the face and chest. A number of the physicians of the place came to his assistance. The usual remedies recommended in such cases were resorted to, but without any mitigation of the urgent symptoms. The patient was failing rapidly under the increasing spasmodic action of the whole muscular system. It was now determined to administer chloroform, as death was apparently certain without some relief. One drachm of chloroform was put upon a silk handkerchief, and the patient directed to inhale it. The effect was decisive. The patient (who was at this time in a sitting posture, held so by assistants, who could not move him in the least degree without exciting the most frightful and alarming spasms) requested to be placed in a recumbent position, which was done without exciting the least spasm. The chloroform was carefully administered for some hours, the patient holding the handkerchief most of the time himself, in order, as he said, "to keep off the dreadful spasms." From this time he recovered rapidly, and on the 7th instant was able to leave for home a distance of six or seven miles.—*Boston Med. Jour.*—*Med. Gazette, Aug.* 2, 1850, *p.* 187.

153.—INTERESTING OBSERVATIONS ON THE SUBJECT OF VEGETABLES.

By ARTHUR HENFREY, Esq., F.L.S., &c., and Dr. SCHLEIDEN.

The chemical elements in vegetable bodies are of two orders; the inorganic and the organic. The inorganic are those which were formerly called ultimate principles, because their existence was shown only by ultimate analysis and destructive decomposition. These are, carbon, hydrogen, oxygen, and nitrogen, in the first place; and, in smaller proportion, and less generally distributed, chlorine, iodine, bromine, sulphur, phosphorus, silicon or silica, potassium, sodium, calcium, magnesium, aluminum, iron, manganese, copper. Of these, carbon is the most abundant, and carbon, hydrogen, and oxygen exist in all plants; while nitrogen is found only in certain orders, especially the fungi, the seeds of several of the Cerealia, the leaves of tea, and the berries of coffee.

Of the origin of these elements in plants, only one opinion is at present entertained, namely, that they are taken up from without, although some speculators, influenced probably by the love of the marvellous and the mysterious, maintained that they were in some mode formed by the organs of the plant. All these substances are either conveyed into the plant through the medium of the water surrounding it, or through that of the air.

The inorganic elements, combined in various modes and proportions, form the organic elements or proximate principles, as they used to be denominated. These are, 1. cellulose, or sklerogen; 2. amyloid; 3. vegetable jelly, or pectine; 4. starch; 5. gum; 6. sugar; 7. inulin; and 8. oil and wax.

1. Cellulose, sklerogen, or lignine, is the woody matter found in and produced by many vegetable substances. It is principally characterised by this circumstance, that, when treated with caustic potass, or concentrated sulphuric acid, starch is formed. It is abundant in the husks of seeds, the coverings of nuts, and various similar parts of plants.

3. Of vegetable jelly, bassorin, the most obvious example is salep, and, it may be added, the Irish moss or Carageen (Chondrus crispus). When dry, it is horny or cartilaginous; but when moist, it swells and assumes the appearance of jelly, more or less tremulous. It is insoluble in fixed and volatile oils, ether, and alcohol, and is not coloured by iodine. It passes, on the one side, through various transitionary states into Cellulose, through the cell walls of the *Fucoideæ*, and into amyloid; and, on the other, into starch, and in many modes into gum and sugar,

In the same manner as in animal chemistry, a distinction is made between gelatine and gelatinous substances; so Kutzing distinguishes between vegetable jelly and Gelin, the former of which, by boiling. passes into the latter. Vegetable jelly also, by long boiling, passes into *Protoplasma* or gluten. These three principles Dr. Schleiden regards as hydrated states of one common fundamental principle. He thinks it doubtful whether pectin and pectic acid ought to be placed under the head of Jelly.

4. *Starch. Amylum.*- The characters of this substance are well known. Varieties are lichen starch, potatoe starch, wheat or cereal starch, arrow root, sago, and many others.

Starch is the most generally distributed principle in the vegetable kingdom; for there is scarcely a vegetable production in which starch does not form a portion more or less considerable in its growing and grown life; and in a great number of vegetable bodies it is most abundant. This extensive and copious distribution of starch is evidently connected with the uses of the vegetable world as means for furnishing food to man and animals. Many vegetable substances, —both roots, tubers, stems, and seeds, owe their power of furnishing food to the quantity of starch which they contain, and may be made to supply. Dr. Schleiden thinks it is no exaggeration to assert, that starch constitutes the most important and the almost exclusive food of two-thirds of the whole human race. Certain parts of plants contain it in the largest quantity, namely, the albumen of the seeds of the Cerealia, the cotyledons of the *Leguminosæ*, the medulla or pith of the stem in the *Cycadeæ* and Palms, the bulbs of the *Liliaceæ*, the tubers, root-stocks, and roots of different families of plants.

Though starch has been the subject of many researches by different inquirers, yet on few subjects are there prevalent so many opposite and conflicting opinions It was the opinion of Poggendorf in 1836, that after ten years of investigations we are little or nothing in advance of the old views; and at present Dr. Schleiden entertains nearly the same idea.

5. *Gum* (Arabin, Dextrin, Vegetable Mucilage in part).—Its appearance and characters are well known. It is found in a state of solution in the interior of cells, or as a secretion in the large gum canals; and not unfrequently mixed with vegetable jelly, and is frequently, by means of foreign substances, coloured yellow or brown. The purest is Gum Arabic. It is found in large quantity in the *Mimosæ* and *Cycadeæ*.

Dextrin, which can be formed by the action of dilute sulphuric acid, diastase, &c., in cellulose or starch, agrees in many points with gum, and especially in elementary composition. According to Mulder, the greater part of what has hitherto been called gum is dextrin. Some time back, namely, in 1843, Dr. Schleiden advanced the opinion, that dextrin must be present in plants where so much cellulose and starch was dissolved and changed. Soon after, Mitscherlich pointed out the actual presence of dextrin in the sap of many plants. The principal difference between gum and dextrin consists in the fact, that the latter, by the action of dilute sulphuric acid or diastase, is converted into grape sugar, while the first is not. Gum, the author thinks, originates in the plant from dextrin, and is not formed as a special product of secretion; whilst dextrin is present in all the juices of the plant, and especially where cells are about to be formed, and it appears to be the formative matter of the plant. The modifications of dextrin through vegetable jelly till it forms cellulose, are almost innumerable. The transition relations of dextrin may be thus stated;—dextrin, gum, vegetable jelly, cellulose.

6. *Sugar*.—This well known substance is most widely distributed through the vegetable kingdom, and especially where starch and the other substances are in the process of development, or are being dissolved; as in unripe peas, and the cereal grains; and the early sap of trees, as of the maple and beech. It is found in greater quantity and in a more permanent form in the stems of grasses, as the sugar-cane and maize, and the *Holcus saccharatus;* in fleshy roots, as the carrot, parsnip, and beet; and in juicy fruits, as the pear and apple, gooseberry and currant. Naturally it is found dissolved in the plant; but when it is excreted, it assumes the form of crystals, as in the nectaries of plants,

for instance, *fritillaria imperialis.* Mannite, the sugar of manna, he regards as not belonging to this series of substances.

7. Inulin,—of which varieties are Dahlin, Calendulin, Synantherin, Sinistrin, –is obtained from the tubes of the Dahlia by simple washing. It is a powder, with fine grains; the grains clear, easily soluble in boiling water, from which it separates, on cooling, in a granular form. From the atomic proportions given by the analyses of Mulder and Crookewitt (C^{12}, H^{10}, O^{10}) as to the Inulin from the Dahlia, Helenium, and the Dandelion, it must be isomeric with sugar and starch. Inulin is found in many parts of the vegetable kingdom, where formerly starch was supposed to be contained, as in tubers and fleshy roots; for instance, *Inula Helenium, Dahlia variabilis;* and Dr. Schleiden thinks it probable that it is widely distributed.

8. *Fixed Oils and Wax.*—Of the latter substance, not much is accurately known. It is evidently very extensively diffused. In all those plants and parts of plants called hoary, and covered with down, the delicate bluish bloom consists of a thin layer of very minute wax granules. One of the best examples is the bloom or the down of the peach. Wax, he adds, decidedly, is formed by bees out of sugar. Is it not collected by the bees from the parts of flowers?

9. A class of substances is found in plants which neither exist in the cell walls, nor are the cell walls formed from them; yet their presence is necessary for the simplest processes of vegetation. Besides the three usual principles of carbon, hydrogen, and oxygen, nitrogen is added, and sometimes phosphorus and sulphur. To these substances, which the chemists distinguish by the several names of albumen, gluten, gliadin, zymon, gelatin, diastase, vegetable gluten, legumen, &c., Dr. Schleiden applies the general denomination of Mucus (*Schleim*). Hugo Von Mohl has named it *Protoplasma,* from its resemblance to albumen. Some of the varieties now named, Dr. Schleiden thinks, may be the result of some process of separation from the plant. According to chemical properties they have been distinguished into three groups;—vegetable albumen; fibrin, the gluten of the *Cerealia;* and Casein, which is the legumin of beans, peas, and other seeds of the *Leguminosæ.* To these, Dumas adds a fourth, namely, Animal Gelatine, which, however, Schleiden thinks should be regarded as part of the composition of gluten. Mulder shows that all these substances have one common basis, which, in the same manner as in animal chemistry, he denominates *Proteine;* and he infers, that proteine, combined with sulphur, forms casein; with phosphorus and sulphur, fibrin; and with a larger proportion of sulphur, albumen. Mulder further observes, that these substances cannot be formed in the animal body, but must be introduced from without.

The principles or substances now enumerated are, during the life of the plant, incessantly passing from one to the other; and for this object the presence of mucus or albumen in the cells appears to be necessary. All are carried, suspended, or dissolved in the juices of the plant; and though some of them may co-exist at the same time in these juices, yet several of them appear to succeed each other in regular and uninterrupted series. The substance which to-day is amyloid, dextrin, and vegetable jelly, may, in the course of a few days, be sugar, starch, or gum. Dr. Schleiden thinks that it is impossible to regard them as well-defined or distinct forms of matter, since they are thus mutually interchangeable. It is possible, also, he observes, to produce this series of bodies by mixing them with mucus, that is, vegetable albumen, or acting upon them with sulphuric acid or alkalies, or even by repeated solutions or evaporations. But this mode of expressing himself is not accurate; and what he means, we conceive, is, that any one of these bodies may be converted into the other by the use of sulphuric acid and the alkalies, or by such chemical processes as repeated solutions and evaporations. To the property which albumen, sulphuric acid, and similar agents thus possess of producing in other bodies chemical changes without themselves becoming changed, Berzelius gave the name of *Catalysis,* and Mitscherlich, that of *contact-action.*

It has been supposed that these peculiar changes depend on the operation of that principle which has been assumed to pervade living bodies, namely, the Vital. This, Dr. Schleiden is not very willing to admit, and he speaks of it as a principle or property, the existence and agency of which requires yet to be proved.

All that can be said in reference to the present subject is, that we know with certainty that, without the plant, cellulose passes into starch, that starch passes into dextrin, dextrin and cane-sugar into grape sugar, and grape sugar into gum, as in the fermentation of beet-root juice. We further know, that while the first process, that of the transition of cellulose into starch, is effected only through the agency of sulphuric acid, all the other metamorphoses can be produced without the plant, by the agency of nitrogenous substances, as albumen. It may therefore be concluded, with a considerable amount of probability, that within the bodies of plants sugar passes into dextrin, dextrin into starch, amyloid, cellulose, and vegetable jelly, that a transition takes place from wax into sugar, from sugar and starch into wax, from starch into fixed oils, and from the fixed oils into sugar and dextrin. All these changes depend chiefly on the reception or the deposition of oxygen.—*Edinburgh Med. and Surg. Journal, October,* 1850, *p.* 370.

154.—ON THE CELLULAR STRUCTURE OF PLANTS.

By ARTHUR HENFREY, Esq., F.L.S., &c., and Dr. SCHLEIDEN.

To those conversant with the history of botanical research and the scientific study of the structure of plants, it is well known that Robert Hooke, the earliest of English micrographers, was the first discoverer of the cellular structure of plants. He began his researches in 1619, when he received a microscope, brought to England by Cornelius Drebbel; and he appears to have beheld the cellular arrangement in 1620. After him came Marcello Malpighi, professor of anatomy at Bologna. He not only observed the celluliform structure of plants, but represented it to be composed of separate cells, to which he applied the name of *Utriculi.* Nehemiah Grew, who, during the time of Malpighi, was secretary to the Royal Society, was aware of the cellular formation; but he makes some mistakes as to the structure of their walls, which more careful observation would have enabled him to avoid.

It is singular, that though glimpses of the celluliform structure were thus early obtained, yet for a long period, nearly two centuries, it may be said, no step was taken to study the structure fully and systematically. It was only in 1833 that Robert Brown, in an interesting memoir on the *Orchideæ*, made known the existence of the cytoblast or cell-nucleus as a body frequently present in plants. He gave it the name of nucleus of the cell In 1835 Hugo Von Mohl made known the multiplication of the vegetable cell by division in the instance of the stomata in the linæa; but this contained several imperfections, which were rectified by subsequent observation.

In 1838 Schleiden first described accurately the vegetable cell, and first pronounced its formation to be the fundamental principle in botany. By plant cell, he tells the reader, he understands exclusively the elementary organ, which, when fully developed, possesses a wall formed of cellulose and a semifluid nitrogenous content, as constituting the essential element of form in all plants, and without which the object is not a plant. These cells, for they are always multiplied, can only be formed in a fluid which contains sugar, dextrine, and albumen, (protoplasma or proteine compounds), in short, a nitrogenous element. The proteine compounds, he thinks, are the primary agents in the process as in the chemical transformations. He specifies two circumstances as requiring to be distinguished.

The *first* is the formation of cells without the influence of another cell previously existing, as is exemplified in fluids capable of fermentation. A globule of nitrogenous substance first arises; in this a cavity is formed; it grows; and the complete cell has a delicate coat of cellulose or lignin, without our being able to determine the period at which it is produced.

The *second* circumstance is the formation of cells under the influence of a complete cell already existing, or the multiplication of the plant-cell. The mode of multiplication of vegetable cells appears not to follow the same type in all cases. He distinguishes two sorts of multiplication.

First, the *Protoplasma* or nitrogenous substance is collected into a more or less perfectly spherical body, which is at length sharply defined, and forms the nucleus of the cell, or cytoblast. Upon this is deposited a layer of protoplasm or albumen, which expands as a vesicle, and forms the subsequent lining of the cell At a very early period the whole becomes enclosed by a wall of cellulose, and the cell is completed. This mode of multiplication appears to take place especially in the embryo sac and the embryal vesicle.

In the second mode, the whole contents of a cell, including the nitrogenous lining, are divided into two portions, which appear to be separated by a lighter zone; and around each portion is formed a wall of cellulose. The nucleus may, in this case, either be divided so as to form two *nuclei*, each of which becomes the central point for one of the cell-forming portions of the content; or it may disappear, while a new nucleus is developed in each of the new cells after their production.

This second mode of multiplication appears to occur in all the other parts of the plant.

The cytoblast is met with in all newly formed cellular tissues; but at a later period it disappears from the same cells. It is seen in various stages of progress to perfection. When perfectly formed, it is a flat, lenticular, sharply defined, transparent, pale-yellow body, in which it is easy to distinguish one or two, seldom three, sharply defined and evidently hollow corpuscles, which are called *nucleoli*. In its most imperfect form, it appears merely as a flat, yellow, semigranular globule, in which are no *nucleoli*, and in which nucleoli do not subsequently appear. It varies in characters according to the plant, as well as its age; in colour, from perfect clearness to a dark yellow grey; in consistence, from granular albumen to a firm homogeneous mass; in the number of its nucleoli, from one to three; in the form of the *nucleoli*, from total absence to a hollow globule; and in its own shape, which may be globular, lenticular, or egg shaped; and in the degree of attachment to the cell wall, from loose adhesion to perfect union with the cell wall and enclosure in a duplicature of the same.

"When the cytoblasts are perfectly formed, they soon present a delicate membrane, which encloses them, and which is sometimes extremely fine and soft, and sometimes thicker and more compact. This membrane soon becomes elevated on one surface of the cytoblast in a vesicular form, and gradually extends itself, so that the cytoblast occupies at last a small part of the wall; but still the cytoblast continues to enlarge, and the nucleoli become more evidently defined. The membrane of the vesicle, or young cell, becomes gradually stronger and thicker; it gets a round, or sometimes an elongated form, and sometimes an irregular edge, which subsequently disappears."

Another mode of cell formation has been pointed out by Naegeli, who observed it first in the primitive cells of the pollen, and subsequently found it in a large number of Algæ. This Dr. Schleiden has not himself hitherto seen; but he gives the following statement from the description of Naegeli.

"So long as a cell is internally covered with a layer of mucus, this process may go on. In the first place, this mucus layer is divided into two or four parts, each of which is surrounded by a delicate layer of mucus. These external mucus layers are converted into cellulose, and thus two or four little sacs or cells are formed, which perfectly fill up the primitive cell. In a peculiar and hitherto unexplained way, the cytoblast seems to be very active in this process. This increase takes place in most instances in cells with a central cytoblast, and this divides itself into two or four cytoblasts, each of which becomes the central point of a new cell."

No objection, Dr. Schleiden thinks, can be made to this history of cell-formation, founded on observations so careful.

To this theory, nevertheless, Mr. Henfrey urges the objection, that there is no explanation of the mode in which the albuminous sac or primordial utricle becomes divided; and the evidence of the simultaneous deposition of the septa is insufficient. This, he adds, will become apparent in the consideration of the next theory.

He then states, as a third mode of formation of new cells, that it commences by the doubling inward of the primordial utricle, so as to form an annular or ring-shaped channel. This gradually increases in depth till it divides the utricle into two portions, each a perfect closed sac. The unfolding of the primordial utricle is followed by a deposition of permanent cell-wall from each lamella.

What is the use of the nucleus, what function does it perform? is a natural question. This point, nevertheless, has not been determined. Mr. Henfrey thinks with Mohl, that it is intimately connected with the chemico-organic changes of the cell-content, and that it is concerned in the elaboration of the mucilaginous or rather plastic granular matter, which is named by Mohl, *protoplasma.*

Our limits permit not to give all the facts adduced by the authors on the process of cell-formation and the different modifications which it undergoes. But we subjoin as general inferences the following remarks:—

"Up to the present time no fact has occurred which is not in accordance with the complete precedence of the cytoblast, as above observed. The indications of its precedence are only obscure and incomplete in those cases in which accurate observation is surrounded by insurmountable obstacles. It is in the formation of the spores, the foundation of the future plant, in *Crytogamia;* in the embryo, the young plant itself, of the *Phanerogamia,* that the precedence oft he cytoblast is fully made out. Both serve as points of support for analogous conclusions in other cases; and it appears, until further researches may necessitate modification, that we may safely conclude that the precedence of the cytoblast in the formation of cells is a universal fact.

"If, further, we regard the easy transformation of the assimilated matters, and may from artificially conducted experiments draw the conclusion that the nitrogenous matter which I have called mucus, and which forms the cytoblast, is the substance which calls forth these transformations; and if we further remark that sugar and dextrin are more easily soluble than jelly, and that sugar and gum are changed into jelly if the quantity of water is not increased, and which must be necessarily precipitated, we must regard the whole process of cell-formation as simply a chemical act. The gathering together of granules of mucus to form the cytoblast we can as little explain as that, when we form a solution of two salts, if we throw into the mixture a crystal of one or the other salt, that salt alone crystallizes around it."

It is well known that this celluliform structure, which is thus shown to form the basis of the vegetable tissues, Schwann has shown, is extended to animal bodies. On this extension Dr. Schleiden delivers the following commentary:—

"Schwann has pointed out, in an acute and profound treatise, that the animal organism also is composed of cells, and that these cells are formed in the same way as those of plants. If this law is found essential to some plants and animals, this analogy forms a basis for enunciating this mode of formation as a universal law for both kingdoms of nature." —*Edin. Med. and Surg. Journal, Oct.* 1850, *p.* 376.

155.—*On the Therapeutical Employment of Coffee and Cafeine.* By MM. VANDEN-CORPUT and HANNON. —M. Vanden-Corput has recently published an article upon the febrifuge power of coffee, and especially its anti-neuralgic action, on which account it is now very much employed by the Belgian practitioners. Numerous therapeutical applications of this substance were made long since. Nebelius and Baglivi gave it in cephalalgia, Dufour prescribed it in phthisis and migraine. Willis employed it in narcotic poisoning; and Grindel and Dorpat as a febrifuge. Musgrave, Pringle, Monin, Percival, Laënnec, and a great many others, have spoken of it approvingly in essential asthma. In Dutch Batavia it is used in strong infusion, with lemon-juice, in pernicious fevers; and the practice passing thence to Holland, has led to its being preferred there to quinine. Pouqueville declares it is infallible in the intermittents of the Morea; and Martin-Solon approves of its use in the adynamic form of typhoid. Dr. Guyot has recently strongly recommended it in pertussis. Besides medicinal properties, properly so called, it possesses the important one of disguising the taste of various substances, especially quinine, sulph. magnesia, and senna; and if its antiperiodic virtues really exist, it will probably favour the action of quinine instead of impairing it, as has been feared by some. It possesses the power, too, of developing the action of *haschisch*, contradictory as this may seem to its generally acknowledged anti-narcotic properties.

With *Cafeine*, prepared according to Liebig's process, M. Vanden-Corput produces various compounds. Thus the *citrate*, which is very soluble in water, is formed by saturating pure cafeine in a solution of citric acid, and evaporating; or it may be procured by exhausting crude coffee with a very weak solution of citric acid, shaking the liquor with an equal volume of ether, decanting it, and leaving it to crystallize after concentration. Twelve grains are added to 450 grains of sugar, and the mixture given in doses of twelve grains. The *lactate* may be prepared by dissolving cafeine in dilute lactic acid and evaporating, or by treating the infusion of green coffee with lactate of lime, filtering and evaporating. It may be given in sugar. The *malate* may be prepared in an analogous manner, and administered as a syrup by dissolving 4 parts of the malate in 30 of orange-flower water, and adding to it 250 of simple syrup. Cafeine may also be given with *hydrochloric acid*, as in the following formula:—Cafeine 7 grains, distilled water 1350 grains, strong hydrochloric acid 2 drops, syrup 225 grains. Dose, a table-spoonful.

M. Hannon speaks in the highest terms of the employment of the *citrate of cafeine* in *idiopathic migraine.* Ten grains are first made into as many pills, one of which is given every hour for some time before the paroxysm. The dose is gradually increased until relief is obtained; and in one case even half a drachm at a time was given. The dose must, indeed, be large, in proportion to the obstinacy of the case and the length of time between the paroxysms. Large doses are also required in old, feeble, or cachectic patients; and in old cases the medicine must be long continued. It is desirable, when possible, to commence the medicine the evening before the expected paroxysm, when the entire quantity may be divided into several doses; but if it has been delayed until the commencement of the paroxysm, the whole quantity must then be given at once. The expected paroxysm may thus be entirely arrested or merely diminished in severity; but in all cases, save where the disease is sympathetic, it eventually yields.

While upon the subject of *migraine*, we may mention a still pleasanter remedy than coffee, suggested by M. Tavignot, viz., the making several *deep inspirations* in rapid succession. We must observe, however, that M. Tavignot does not understand by the term *migraine*, simple neuralgia of the head, which many writers so designate; but the condition when this is accompanied by a state of physical and moral prostration, during which the blackest ideas assail the patient,—the "blue-devils," in fact, to which the English were once thought on the continent to be especially liable. During an attack of this, which from former experience he was led to believe would continue for twenty-four hours, he was induced by the hope that this condition of the nervous centres might result from a stasis of the blood in the sinuses of the brain, or from imperfect hæmatosis, to take several deep and rapid inspirations; and after a few efforts of this kind, he found himself completely relieved, and able to resume his occupations. Other persons, similarly effected, have been in like manner relieved; but those who have tried the plan in simple neuralgia, have been disappointed.—*Bull. de Théra.*—*Brit. and For. Med.-Chirurg. Review, Oct.*, 1850, *p.* 557.

156.—*On the Origin of Sugar in the Animal Economy.*—Dr. CLAUDE BERNARD has just published, in the 'Gazette Médicale de Paris,' a very interesting paper on the origin of sugar in the animal economy. He states, that since plants can obtain from the soil and the atmosphere only, the principles of the saccharine matter, they must necessarily elaborate the latter within their organs. Is this the case with animals? inquires the author. Do they form sugar by combining its primitive elements? Or, is the sugar with them the product of the digestion of saccharine and amylaceous substances? To settle this question, the author had recourse to four different series of experiments, cleverly combined and conducted; and from these he drew the following conclusions:—1. There is constantly diabetic sugar in the blood of the heart, and in the liver of man and animals, they being in perfect health. 2. The formation of sugar takes place in the liver, and is independent of saccharine or amylaceous food. 3. The generation of sugar in the liver begins before the birth of the animal, and consequently before the direct ingestion of aliments. 4. This production of saccharine matter, which seems to be one of the functions of the liver, appears to be connected with the integrity of the pneumogastric nerves.—*Lancet, July* 13, 1850, *p.* 53.

157.—*On Tannic and Gallic Acids.* By Dr. BUCHNER.—Tannic acid has the fault of sometimes acting too astringently, while its effects are in general limited to the points where it is applied; whether in the stomach, or at the surface of a wound, it immediately forms with the mucus, protein, &c., an insoluble compound, and does not gain access to the circulation. Part of it is, however, changed into gallic acid, which, after the continuous use of tannic acid is found in the urine.

Gallic acid more resembles, in its pharmaceutical peculiarities, catechu than tannic acid. It has an acid rather than an astringent taste. It does not coagulate protein or mucus. It is readily reabsorbed from the stomach, enters the circulation, and is found in the urine. Homburger, of Carlsruhe, employs it in doses of two or three grains in hematuria, in the bloody diarrhœa of spotted fever, and in tubercular hemoptysis, with excellent effect.—*Buchner's Rep.*—*Brit. and For. Med. Chir. Review, July,* 1850, *p.* 277.

158.—ON THE APPLICATION OF CHEMICAL ANALYSIS, AND MICROSCOPIC EXAMINATION OF MORBID PRODUCTS, TO THE FORMATION OF A CORRECT DIAGNOSIS.

By Bransby B. Cooper, Esq., F.R.S., &c.

[After stating the prejudice and distrust which he at first entertained towards the application of the microscope in the investigation of disease, Mr. Cooper mentions one or two interesting cases, which caused him to form a very different opinion, and which convinced him of the immense importance which the microscope may become in the formation of a sound diagnosis in many obscure cases of disease. After describing the anatomical appearance presented on a division of a scirrhous tumour, Mr. Cooper remarks:]

Dr. Hodgkin is of opinion, that scirrhus and all malignant growths, are composed of a series of simple or compound cysts contained within a general envelope. On a minute dissection of these tumours, by laying open the capsule, the cysts are found attached by a peduncle to the interior of the envelope, the body of the cyst being free, although in contact with the surrounding cysts, which sometimes are so adherent to each other as to render it difficult to distinguish their individuality. The arrangement of the malignant cysts, with respect to their outer envelope, resembles very much the relation of the tunica albuginea to the tubuli of the testicle. Dr. Hodgkin believes that the rapidity of the growth of the cysts leads to the ulceration of their common envelope, through which they protrude, and sometimes become strangulated, lose their nutrition and slough.

A cancer-cell, under ordinary circumstances, is large, rounded, granular, and dark-coloured; sometimes the cell will be found to have burst, and the granules to be diffused in the surrounding tissues. These granules seem to have the power of generating fresh growths, so that the disease goes on indefinitely increasing. When the cancer-cells are developed in the fibrous tissue of the body, they are usually smaller, more irregular in form, and appear to be comparatively limited in their growth, by the inextensibility of the tissue in which they are formed; but although slowly, the malignant growth proceeds with certainty, the fibrous tissue becomes implicated, and, perhaps, contaminated by the cancer-cells, and carcinoma fibrosum is established. The epithelium, when affected in a similar manner, undergoes a like morbid change, and epithelial cancer is, I think, a very appropriate name for the disease.

The circumstances under which abnormal effusions result, may arise from any cause. whether remote or immediately exciting, which induces disturbance in the normal condition of the blood itself, or in the vital or physical condition of the blood-vessels, or in the recipient power of the tissues. That local or constitutional deterioration may thus be produced, which is termed by pathologists inflammation, or increased action, and which is inevitably attended by an altered condition of the affected parts.

This state of inflammation may be accompanied by all the physical phenomena incident to the increased action arising from the stimulus of a new growth, or from the reparative action set up when a tissue has been subjected to injury; but still a great distinction must be drawn between this condition and that in which malignant disease becomes developed: in the former cases the natural exciting causes merely produce an increased flow of healthy blood through normally distended vessels to a tissue requiring an additional supply of nutrient matter, either for the new growth, or for the reparation of a normal structure; while, in the latter, the deteriorated condition in the force of nutrition leads to the effusion of products which may themselves become permanent morbid elements within the tissues into which they are effused, and thus constitute all those circumscribed growths which are by surgeons classed under the generic term of tumour. The nature of this new growth may very generally be ascertained by the introduction of a grooved needle, which will bring away quite sufficient of the morbid matter for microscopic examination. These products may prove to be nothing more than hypertrophy of the tissue into which they are effused, and may be productive of no injury, in some cases, not even of inconvenience, as we find in exostoses, steatomata, &c.; or from their size they may interfere with the functions of the surrounding structures, and lead to the necessity of their removal. In other cases the new developments may be of such a nature as to require a constitutional effort, either to expel them by sloughing or ulcerative process, or to produce their disintegration and dissolution into their proximate elements, so that they may be removed by absorption. In some constitutions, where the malignant diathesis prevails, these new elements may have an infinite tendency to increase, and by their continued growth lead to such a depressing influence on the nervous system as to destroy life; some persons thus affected sink from the extension of this indefinite increase to some important vital organ. If the malignant disease be of the nature of scirrhus or hard cancer, it propagates itself locally by the development of cancer-cells; but if of the soft nature, as melanosis or fungus hæmatodes. it seems to be conveyed by the absorbents into the veins, and, the blood being contaminated, any part of the body may become affected, without any continuity of the original disease. Therefore, in the case of hard cancer, when extirpation is performed, the whole of the disease must be most carefully removed, or the remaining cancer-cells will be certain to develope it afresh; in the soft kind, operation can afford but a very slight prospect of permanent success.

There are many microscopists, and I may, without partiality, quote Mr. Quekett as one of the most eminent, (as is sufficiently known to the profession by his beautiful work on 'Histology,' which has just issued from the College of Surgeons, in the humble form of a volume of the catalogue of their Museum,) who are able to determine, by microscopic examination of an effusion, whether or not the product results from a malignant action: on one occasion I took some discharge which I had removed from the surface of a fungoid tumour, and without any previous knowledge of the source from whence it was derived, Mr. Quekett pronounced it to be of a malignant character: in several other instances he has recognised the same indications of malignity, merely in the blood withdrawn from the veins of a malignant tumour.

Not only is much useful knowledge to be acquired from the just estimation of the products of diseased action, from the analysis of their constituents, but at the same time the inquiry should be carried further, for the purpose of discovering whether or not, in the presence of certain abnormal elements, something like a corresponding condition of constitution is not generally present, as, indeed, is already universally acknowledged to be the case in the peculiar effusions which result from a strumous diathesis, the non-plastic products resulting from the inflammation of serous membranes in broken-down constitutions, the unhealthy granulations of cachexia, and the indefinitely increasing development in malignant disease, are all circumstances the consideration of which is calculated to lead, in my opinion, to a more certain method in the administration of remedies, than could be possibly arrived at without such investigations.

These views may appear, at first, more applicable to the practice of the physician than that of the surgeon; but as the latter has every day brought before his notice the proofs of local disorders being modified and affected by the state of the constitution, it is my object to show, that the surgeon who would practise his profession scientifically, must acquire a knowledge of the peculiar condition of his patient's constitution, by the character of the effusions which result in nature's efforts at reparation. In specific diseases, as in syphilis, we find that the local influence of the poison is modified by the constitutional peculiarities of the patient, not only as to the period at which the disease is propagated (termed the period of incubation), but also as to the conditions of the sore itself; as to whether it be indurated, phagedenic, or gangrenous, I am of opinion that such conditions are not dependent upon any peculiarity in the poison itself, but upon the diathesis of the patient; and, indeed, even the uncertainty as to the period of reparation is also attributable to constitutional causes. It is these variations which render the treatment of syphilis so difficult; but it may yet come to pass that by the aid of the microscope, and perhaps by chemical tests, we may acquire a knowledge of the nature of syphilitic virus, and thus be able to learn, whatever may be the constitutional peculiarities, whether or not the patient be the subject of a specific poison. This is most important to ascertain, for if it could be positively determined, the specific antidote, mercury, must be administered to destroy the poison, while the physical conditions of the sore itself might require other constitutional and local remedies.

[Mr. Cooper concludes the subject by relating the cases in his own practice in which the application of these aids have led to an accuracy in diagnosis, which he believes could scarcely have been arrived at by any other means.]

Case 2.—A young lady, æt. 28, called at my house on the 20th of February, 1850, under the following circumstances:—She stated, that fifteen years before she had fallen upon her nates, causing her considerable pain, especially on the right side of the part injured, and that from that period she had perceived an enlargement in the right gluteal region, which had gone on gradually increasing in size. Upon examination, I found a tumour about the size of a child's head, tense and fluctuating, perfectly painless, even upon manipulation, and without the slightest discoloration of the skin. With a small trocar and canula I drew off about ℥xij. of perfectly limpid fluid, as clear as from a mountain stream; the specific gravity was 1008: by analysis it was found to contain chloride of sodium and a small quantity of phosphate of lime, properties so exactly coinciding with those of the fluid of the spinal marrow, that I was at first led to suspect some communication between the tumour and the vertebral column; but upon more minute inquiry into all the symptoms, the perfect absence of any approach to affection of the spinal nerves convinced me that the opinion was fallacious. Upon placing the fluid under the microscope, I soon detected numerous echinococci, which cleared up the case at once, proving the tumour to depend upon the development of hydatids. I determined, therefore, not to interfere further with the tumour, having intended, before I made this discovery, to inject it; but, from my experience in cases of hydated tumours, I have found, that the constitution of the patient remains unimpaired, so long as the animalculæ are alive; and that irritative fever is excited directly they are killed, as they immediately begin to act as any other extraneous matter, and create considerable local as well as constitutional irritation. I saw the lady again in March, when I found the tumour had scarcely increased since I had drawn off the fluid, and that it had remained quite free from pain; but she told me that her face and neck had become covered by an eruption on the day I drew off the fluid, and remained nearly a week, and then disappeared.

Case 3.—On the 20th of April I was requested by Mr. Odling, of Weymouth Street, to see a patient of his, a baker's wife, in Chelsea, who was the subject of perfect paralysis, which had come on a year before, after her confinement. She had also a tumour on the left side of the neck deeply seated under the trapezius muscle; she attributed its formation to a blow (which I have almost invariably found to be the exciting cause of the development of hydatids). The tumour was quite free from pain, nor had the patient ever experienced the slightest rigor. The question was mooted by Mr. Odling, whether or not the swelling might not have some connection with the paralytic condition of the patient. As the fluctuation was distinct, I recommended the evacuation of the fluid, concluding that the contents of the tumour were not purulent, from the complete absence of rigor and constitutional irritation of any kind; I drew off 250 grains of perfectly limpid fluid, which was of very low specific gravity, 1006, and contained a large quantity of chloride of sodium, some phosphate of soda, and a trace of phosphate of lime. I expected, from the appearance and analysis of this fluid, to find that it contained echinococci as in the last case, and therefore placed it under the microscope, but, to my great disappointment, failed in finding any. Ten days after, however, the cicatrix of the opening which I had made gave way, and several large hydatids made their escape, verifying the identity of the character of the two fluids in which the hydatids were found. In the interval of time between the evacuation of the fluid and the discharge of the hydatids, the patient had suffered considerable constitutional derangement, which yielded directly they were expelled; the tumour has entirely disappeared, although the paralysis remains, but Mr. Odling thinks rather less complete than before the operation.

It sometimes happens after the operation of lithotrity, or more frequently from stricture, or from a blow on the perineum, that an abscess may form in the perineum, groin, or even on the upper part of the thigh, when it becomes a matter of great importance to discover whether or not this formation of matter depends upon the extravasation of urine, or merely upon an irritation set up from either of the above causes. The question may be readily decided by evaporating a small portion of the evacuated pus to the consistence of syrup, rendering it quite cold by placing the vessel, in which it is contained, on an ice bath, then adding a few drops of pure nitric acid; and laminated crystals of the nitrate of urea are immediately formed if urine be present.

Case 4.—I have lately had a gentleman under my care, Mr. S., æt. 63, who, 30 years ago, met with an accident, by which his back was injured, an abscess formed just below the last rib, and ever since matter has continued to form and make its escape by the opening. For the last five or six years he has become much emaciated, his appetite is bad, and his bowels very irregular in their action. Lately the discharge from the abscess has been of a bright yellow colour, and since this has been the case, he has apparently lost flesh more rapidly than before. I placed some of the yellow discharge under the microscope, and it appeared to me to be composed of chyle, mixed with granular mucous corpuscles, tinged with bile. Not satisfied with my own observations, I took some of the fluid to Mr. Quekett, who said that he could not be quite certain about the presence of chyle, as the specimen had become somewhat dry, but that there was no doubt of the presence of the biliary matter; besides this, to prove that the fluid had passed from a bowel, he pointed out to me the presence of some half-digested muscular fibre. Since that period I have seen my patient in consultation with Dr. Addison, and we both detected a strong fæcal odour in the discharge from the abscess; this proved that the chronic abscess now, at any rate, communicated with the interior of an intestine.

Case 5.—A gentleman, who had submitted to lithotrity, had an abscess formed in the groin, after he had experienced great pain in voiding a portion of detritus; the surgeon questioned whether or not the urethra had been lacerated by the portion of calculus, and was disposed to believe the abscess had resulted from mere irritation; I tested the pus by the method I have described, and detected urea, which was at once conclusive that the urine had escaped from the natural passage.

Case 6.—A patient at Guy's had a blow on the perineum; a few days afterwards he was seized with rigors and great pain behind the scrotum; the next day a tumour of considerable size had formed, and the patient suffered from retention of urine. I immediately laid open the perineum, and evacuated a considerable quantity of pus, which, upon analysis, gave evidence of the presence of urea; and the true nature of the case, namely, the rupture of the urethra, was subsequently further determined by the urine passing through the opening I had made. I have, however, in two or three cases of blows on the perineum, known abscesses to result, and upon analysis of the pus not found urea to be present; in each of these cases the openings I made healed without any escape of urine through the wounds. The treatment of these cases should be modified by the knowledge thus acquired, of whether the urethra were wounded or not, as, in the former case, the patient should only be allowed to pass his urine through a catheter; while, in the latter instance, the use of the catheter is not only unnecessary, but should be strenuously abstained from, as it would only tend to increase the irritation uselessly. Abscesses in the loins, which are sometimes known to result from lesion of the kidneys or ureters, might, although usually so difficult to recognise, be at once diagnosed by this method of analysis, which should never be neglected where there is any doubt about the subject.

Such chemical indications cannot, however, always be prosecuted with advantage, although they may, in a chemical point of view, promise an effectual remedy. A gentleman, whom I had lithotrised several times, was at last freed from the presence of any detectable portion of stone; and he left London, although suffering from considerable irritation in his bladder. In about two months he applied to me again, complaining of a return of symptoms of stone; he at the same time brought me a bottle of mucus, which, upon analysis, was found to contain upwards of thirty per cent. of phosphate of lime. On his next visit I sounded him, and although I could not discover any stone either with the sound or the lithotrite, I distinctly felt a roughness on the surface of the bladder, as if it had become coated with phosphates, to which I attributed his symptoms. I therefore injected his bladder with a weak solution of hydrochloric acid, which tended to increase the quantity of phosphates rather than diminish them, for I not only found the injection, which I drew off about five minutes after its introduction, containing phosphate of lime, but the quantity in the mucus continued the same, and sometimes even increased. I considered, therefore, that the acid increased the irritation in the bladder, and so created a disposition to the deposition of phosphates from the urine equal to the quantity it dissolved; I therefore ceased to employ it, prescribing liq. potassæ and laudanum, under which treatment the patient perfectly recovered.

A second case, which I treated precisely in the same manner, terminated similarly in every respect; and my experience, therefore, leads me to believe, that little benefit is to be derived from the injection of acids into the bladder in such cases. Still there can be no doubt, that the science of chemistry, aided by the use of the microscope, is more applicable to the diagnosis of urinary diseases than to any other to which the human subject is liable; and I believe that in those classes of diseases in which an abnormal condition of the urine constitutes one of the earliest symptoms, their application is almost indispensable. It is well known to every practitioner in medicine, that very slight disturbance to the health, any trifling functional derangement, is quite sufficient to cause the urine to assume an appearance so foreign to its natural state, that the most cursory observer cannot fail to be impressed with the change. A slight cold, an attack of indigestion, a meal of some particular kind of food, or other accidental circumstance, may be sufficient, in some constitutions, to produce a change in the appearance and character of the urine; but it is equally true, that such a change may result from some severe constitutional disorder, and prove indicative of a state of health most dangerous to the patient's safety. The urine, when in a state of disorder, (either from the trifling causes alluded to, or in consequence of the more serious deterioration) amongst other changes, often loses its normal transparency and colour, and becomes loaded with a more or less abundant deposit of an opaque solid nature. Now without entering into the subject of the peculiar changes to which the urine is liable under different forms of disease, or of the indications furnished by these abnormal characters, it will be sufficient for the purpose of proving the applicability of chemistry and the microscope to take the case of urine containing a visible opaque deposit. Such a deposit may arise from many different causes, and may consist of matters differing extremely from one another, and indicative of totally opposite conditions of the constitution; it must, therefore, be very important to be able to distinguish these various deposits from one another, inasmuch as the treatment of the patient must be regulated according to the nature of the urinary deposits, as pointing out, very generally, the origin of the evil, as to whether it arises from trifling functional disorder, or from severer constitutional lesion.

So much do these urinary substances vary, and still sometimes so resemble each other in appearance, that it is often extremely difficult to distinguish between them by the ordinary means of observation; and I think I may venture to state, that, under certain circumstances, it would be impossible, by the unaided senses, to determine whether such a deposition consisted of uric acid, urate of ammonia, phosphate of lime, mucus or pus; for when examined by the naked eye, the deposits of any of these ingredients vary but little in character, at any rate afford but a slight guide to medical practice. The case is widely different when such deposits are subjected to investigation by means of the microscope and chemistry. Under the microscope, that which was confused and amorphous resolves itself into characteristic forms, easily recognisable, and indeed unmistakeable to the practised eye. In some cases, however, in which form is not always sufficient to satisfactorily decide the point, as may occur in deposits of phosphate of lime, urate of ammonia, or uric acid, chemistry steps in with her menstrua, and at once determines it beyond any further question. To illustrate the advantages of being able at once to ascertain the nature of a urinary deposit, let me advert briefly to the different circumstances under which deposits of totally different characters may be produced, although, to a certain extent, presenting the same appearance to the naked eye. We have deposits formed in the urine under certain constitutional conditions almost diametrically opposite to each other; checked perspiration, slight dyspepsia, excess of nitrogenised food, an attack of fever, and other causes of a similar nature, will probably produce the determination of an excess of nitrogenised matter to the kidneys, to be ejected from the system in the form of uric acid, or urate of ammonia. On the other hand, phosphate of lime seems to be directed in excess to the urinary organs when there is not sufficient vital power in the system to carry it to those tissues which normally require a certain supply of this compound; when, therefore, phosphate of lime is found in the urine, we may expect to meet with great constitutional and nervous depression in the patient, which is a condition that yields with great difficulty to the influence of medicine: the presence of the triple phosphate is also indicative of the same condition. It is by the aid of the microscope and chemical tests only that these deposits can be distinguished from one another, and thus their forms, crystalline or otherwise, can be appreciated under the microscope, while their relation to chemical solvents affords an unerring guide to their true nature; which, being ascertained, the diagnosis and appropriate treatment is rendered comparatively easy. For the just estimation of the value of these facts, I cannot do better than again revert to my friend Dr. Golding Bird's most excellent work on 'Urinary Deposits,' which is equally adapted, from the depth of its research, to benefit the established practitioner and the student in medicine.

I had a case related to me lately by Mr. Iliff, jun., of Newington, which, although from the character of the disease, it may not appear a very important one, still bears so strictly on the subject before us, viz., the use of the microscope in arriving at a just diagnosis, that I shall relate it. Mr. Iliff had a patient who was the subject of a cutaneous eruption, which, in some respects resembled scabies, but still, from the high degree of erythematous inflammation, there was some doubt on the subject; Mr. Iliff, however, applied the sulphur ointment, which directly so aggravated the diffused inflammation of the skin, that he discontinued it, and consulted Mr. Erasmus Wilson, who had also at first some doubts as to the nature of the disease, but settled the question at once by the following plan: with a needle he scraped off the cuticle from one of the pimples, and then insinuating its point into the base of the elevation, he soon withdrew the little animalcule, which, although scarcely discernible by the naked eye, when placed under the microscope, was at once recognised as the acarus scabiei, which immediately settled the question.

The detection of spermatozoa in the urine may indicate the propriety of a change of discipline and treatment that may snatch a patient from the jaws of death, and this source of depression could not be detected without the use of the microscope.

In conclusion, I must beg leave to impress upon my readers that by this commendation of the use of the microscope and chemistry, in relation to diagnosis, I have had no intention, whatever, of inculcating any neglect of the usual means which are employed in the investigation of constitutional symptoms, the due performance of the natural functions, and local appearances, as a means of detecting the sources of disease;

but would merely show, that as the sound in the detection of a stone, and the stethoscope in the examination of the diseases of the chest, are the physical means of improving the sense of touch and hearing, so may the sense of sight be exalted by the microscope, while the science of chemistry is competent to the development of facts which the senses themselves are totally incapable of arriving at. I remember when the stethoscope was first introduced in this country, an excellent physician, although perhaps an old fashioned one, ridiculed the use of the instrument, and said, " I never could be an advocate for the introduction of an instrument as an aid to the practice of medicine, which has a tendency rather to substitute the senses for the sense of the practitioner;" but this was, indeed, a short-sighted view, as the stethoscope is only intended to be employed as a means of extending the sense of sound, thus rendering that avenue to the brain more susceptible to the impressions arising from the morbid indications, than the unaided ear could ever be. Such, indeed, is the only use I would wish to propose, of the microscope and chemistry in medical practice, not with a view, indeed of shortening the road to the formation of a just diagnosis, but to render its discovery more certain and unerring; and as the mathematician employs certain formulæ in proof of the correctness of his calculations, so may the use of chemistry in many cases be called in aid by the pathologist, as the test of the accuracy of his previous deductions.—*Guy's Hospital Reports, Vol.* VII., *p.* 105.

159.—CHEMICAL REMEDIES—CAUSTICS—CAUTERIES.

By Dr. DOUGLAS MACLAGAN.

[These remedies, says Dr. Maclagan, may be used for their mere primary or chemical effect to destroy texture, and in this view they have been applied practically to the following purposes:]

To open Abscesses.—There can be no doubt that in nineteen cases out of twenty the proper way to open abscesses is by the knife, as it is equally effectual, more expeditious, and less painful than caustic; and where the matter is at all deeply lodged, in short, where there is any more over it than integument, escharotics as a means of opening it are out of the question. The principal indication for the use of the caustic (the cautery is never used for this purpose) is where a large portion of the skin has become attenuated and discoloured, all of which must be got rid of before a healing sore can be formed; and this can be more readily accomplished by the destructive action of the caustic than by trusting the case to the slower process of ulcerative absorption. I do not know that I can state this indication more precisely; but what I have been generally guided by is this—if the attenuated skin is of a circular form, so that when divided by the knife in any direction it must leave broad, thin flaps, which will be long of ulcerating away, I then employ the caustic, though I must say that occasions for preferring it to the knife are rare. It is not requisite to rub the caustic over the whole surface of the skin which we wish to destroy; it is sufficient to apply it round the edge where the healthy and the thin skin meet, and the whole of the latter being, as it were, included in a ring of eschar, will perish. This plan answers very well in large buboes, especially in scrofulous subjects, where the skin is apt to partake largely of the unhealthy character. When, however, the attenuated portion of skin, though large, is long and narrow in shape, the knife is to be preferred; here you will not be troubled with broad flaps of unhealthy skin; and, remember that, by using the knife, you always give your patients much less pain, though patients will not always be persuaded of this till they try it, and will sometimes, from a foolish horror of anything that savours of an "operation," allow you to inflict upon them a great deal of unnecessary suffering, rather than permit you to relieve them in a few seconds by the bistoury. If the abscess has opened spontaneously, and there is much unhealthy skin, and also unhealhty granular or cellular tissue below it, the caustic had better be used at once, as all this has to be got rid of before a cure can be accomplished. Under such circumstances, thrust the piece of caustic into the opening, turn its point up so as to rub the skin on the inside, thrust it once or twice into the unhealthy tissue beneath, and you will thus effectually destroy all that requires to be removed.

To destroy Morbid Growths.—This is not the proper place for discussing what forms of morbid growths may be treated by extirpation, and what are the circumstances under which we ought to refrain from interfering surgically, and restrict ourselves to palliative treatment. When, however, in accordance with surgical principles, it has been determined that an attempt at removal shall be made, three methods of operating present themselves,—mechanically, by the knife or by ligature, and chemically by cauterants. I think that it may be laid down as a general rule that, if we except such matters as warts and corns, which fall generally under the domain of domestic surgery, there are but few cases where we can recommend the cauterant as a substitute for the knife in the removal of morbid growths. It has no advantage in certainty of effect; it is not so safe as the knife is in the hands of a person of proper dexterity, and competent anatomical knowledge; the wound left by it, when its proper action is over, will not heal so readily as that made by the scalpel; and, therefore, ever since surgeons became more fortified with means of arresting bleeding from wounds, the cauterants have been very generally superseded by the knife, and in some cases the ligature, for the removal of morbid growths. There still, however, are found in surgical works, and still more in empirical surgical practice, statements to the effect, that some forms of local disease,—as, for instance, open cancer,—may be removed by caustic, where the knife cannot be employed. I think no one can hesitate to say that this is against all true surgical principle. When a disease, either from its nature or its situation, is unfit to be subjected to the knife, *à fortiori* it is unfit for extirpation by an agent which must of necessity be more or less uncertain in the extent of its destructive operation; and although, perhaps, now and then a malignant-looking sore has been healed in this way, yet in by far the greater majority of instances the disease is either directly aggravated, or only very partially benefited and above all, it is obvious that no proceeding of this kind can give any better chance to the patient of exemption from the recurrence of the disease, or its attacking neighbouring parts, which is the great impediment to operative interference. I have, for instance, seen small cancerous ulcers of the lip cured by caustic. I saw some of these treated in this way by chloride of zinc when it was new as a caustic, and, therefore, for a while fashionable, and esteemed wonderful. I have also seen some of these sores of the lip which were treated by a druggist's shopman in the country. I had no opportunity of tracing any of his cases from beginning to end; but I mention this man's practice both on account of the destructive agent used by him —powdered arsenic—which, however, is perhaps hardly a true caustic,—and the means which he employed to prevent the poison getting into his patients' mouths. He used bird-lime for this purpose, making a little hollow disk, into which he put the arsenic, and stuck it firmly on the part, leaving it in contact for about six weeks, when a slough came away and the ulcer healed. Of course, I do not recommend this practice for your adoption; not that it was unsafe as regarded the arsenic, for I never could ascertain that any of this man's patients were ever affected with symptoms of poisoning, but that, in reality, there was nothing in its success, such as it was, to recommend it; for a V shaped incision would have removed and cured in a few days any of the cases which I saw; whereas, when he did succeed, he took at least two months to effect it. I have also seen the actual cautery applied to malignant ulcers under the auspices of a high surgical authority in a continental school, and the clinical lesson which I then got was one which will serve me for the rest of my life. I saw three cases of open cancer deeply burned with the cautery, -a proceeding horrible enough to witness, but still more so when the result was seen. All three died from this operation, and two at least of them were affected with purulent deposits in internal organs.

It may, however, be legitimate to use the caustic in malignant disease, where the tumour, from its mere bulk or situation, is such an annoyance to the patient that the surgeon removes it so far as he can as a palliative measure. He may then apply a caustic to the base of the tumour, either to arrest hemorrhage, if troublesome, or, if he is compelled to leave part of the tumour, to prevent its speedily growing again, and keep it in check. I have seen such cases occur, but they are very rare, and must be looked upon as deviations from ordinary surgical rule for a special palliative purpose.

There is one form of morbid growth to which the caustic is generally held to be particularly applicable. I allude to scrofulous tumours, especially when they have passed on to that unhealthy suppuration which frequently occurs in the course of them, and which leaves fistulous sores or flabby ulcers surrounded by hardened scrofulous tissue, in which it is very difficult to produce any approximation to a healing action. In such a state of matters destruction of the diseased tissues by caustic is an established mode of treatment. When it occurs in the ordinary lymphatic glands, in situations where a cicatrix will not cause much deformity, the caustic locally, with constitutional treatment, answers well; but when we wish to avoid having a large cicatrix, as on the neck, we eschew caustics, and try to obtain a less deforming, though more tedious cure, by stimulant applications, conjoined with constitutional remedies When more important glandular organs are affected, as the testis and mamma, the caustic freely applied will eventually produce a cure; but it has appeared to me that, in many such cases, where the organ has been extensively invaded by the disease, even though we feel confident that a cure will ultimately follow, it would be far better to have recourse to the knife. I have seen, for instance, a scrofulous mamma in an elderly woman the subject of consultation. It has been urged, and with truth, "That should heal sooner or later; apply the potash freely and you will cure it." But, in such a case, what is the ultimate result?—a cure, no doubt, but at the expense of much suffering to the patient, and ultimately leaving a breast which, even if the woman were within the child-bearing period of life, would never be of any use to her as a nurse. It appears to me that, when we can cure such a disease with caustic, only by leaving the useless remains of a mamma behind, it is a shorter process, and one entailing no greater ultimate injury, to excise the diseased organ. The same applies to the testicle when it is extensively diseased, and is more frequently acted upon with regard to it; and it also holds true of scrofulous disease in other external parts. It is true that an operation is to be regarded as the last resource of surgery, and should not be undertaken when there is a reasonable prospect of a cure without it. It is also true, that even very bad cases of external strumous disease may be cured by caustic; but it is always to be considered, in following this practice, whether, what we save by such a cure is to be of any use to our patient, and whether it will ultimately be worth all the protracted suffering which has been undergone in obtaining the result.

There are some forms of local disease which have no pretensions to be called malignant, but which are very intractable, being slow to heal and apt to spread, which it would be well if we could extirpate bodily, and substitute for them a clean wound, but which, from the extent of the surface involved, and the situation of the parts affected, admit of no such proceeding. The troublesome disease of the face called lupus, and some of the forms of chronic ulcer, belong to this category. To these cases caustic is generally applicable; the object in using it, however, being, not to extirpate the diseased textures, but rather to alter the action of the parts, and thus lead to the formation of a sound cicatrix. The absorption of solid matter effused into and below the skin is probably one of the chief beneficial effects of the application of the caustic to such cases; —the indurated edges and base of the ulcerated portion are thus removed, and cicatrisation can then go on.

There is one form of morbid growth which I think may be treated with the cautery more than it usually is.—I mean the erectile tumours of the skin in children. These, which consist essentially of a congeries of enlarged blood-vessels, are commonly treated by ligature; but this causes a good deal of suffering, and often much constitutional irritation, before the strangulated mass finally separates. The method of applying the cautery is to pass a large red-hot surgical needle, which may be readily heated by a gas flame, once or twice through the base of the tumour, and the object is to cause effusion of lymph, and thus to alter the structure of the tumour and arrest its growth. There are other means of effecting this object; but I mention this as one which, though noticed in surgical works, has not received that attention which I think it deserves.

To complete the Destruction of Sloughing Textures.—Thus, in phagedenic ulceration, as in cancrum oris, phagedenic chancre, and the bad action which so often attacks sores in hospitals, thorough cauterisation of the parts ought to be had recourse to promptly. It not only expedites the separation of the sloughs which have already formed, but, by a kind of allopathic action, substituting a chemical for a morbid vital process, it arrests the progress of the affection, and gives a healthy character to the sore. In these cases the fluid caustics, such as strong nitric acid, are the best, as they penetrate into all the nooks where the diseased action has established itself.

To prevent the Effects of Poisoned Wounds.—Cauterants are also used on account of their chemical agency in cases of poisoned wounds, and in the early cases of chancre, which in this respect is regarded by many as a kind of poisoned wound. According to Ricord and others, if what is called a true venereal chancre is thoroughly cauterised during the first three or four days, constitutional symptoms never follow; and this is explained by saying, that the surface secreting the poison is thus destroyed, and the virus is prevented from getting into the system. Whether this be the true explanation or not, I need not stop to discuss; there can be no doubt that free cauterisation is the best treatment for primary sores, whether during the first three or four days or later, and all the more so if the edges and base be indurated; and when the slough separates we generally have a healthy sore left, which heals under simple treatment.

With regard to poisoned wounds, as those from dissecting punctures, and by the bites of snakes and rabid dogs, there appears to be good reason to think that the caustics are useful, both by their chemically destroying the organic poisonous matter, and by their cauterant action closing up the vessels of the part, and thus preventing absorption. The use of the caustic does not supersede the necessity in such cases for trying to prevent the ingress of the poison into the system by suction of the part, encouraging it to bleed, or by excision of the whole tract of the wound. The fluid caustics have been recommended as being likely to reach thoroughly to the bottom of these punctured wounds; but, if the injury be of such a form, it is better to dilate it freely, and thus get room for applying a solid caustic efficiently over every portion of the wound.

To arrest Hemorrhage.—I have already told you, that when a cautery or caustic is applied to a raw surface, such as a wound, there is coagulation of the blood or other fluids on the surface, and that the subsequent effects are the same as when the chemical agent is applied to unbroken skin,—that is, there is shrivelling up of the texture, the formation of a slough, subsequently the occurrence of inflammation in the contiguous parts, and lastly separation of the slough. In the arrestment of hemorrhage by the cautery or caustic, all these actions come more or less into play; first the blood on the surface is coagulated, and thus so far forms a mechanical impediment to its further escape from the vessels; but this would be of little avail were it not for the further action of the cauterant on the divided vessels themselves, which, becoming shrivelled and contracted, offer a still further impediment to the flow of blood. Most probably the blood in the vessels of the cauterised part is coagulated also; and thus the primary effect of the cauterant appears to be, to form a clot both without and within the vessel, at the same time that it contracts the open orifice, and thus the flow of blood becomes arrested. But it is clear that so soon as the eschar separated, the bleeding would be renewed, did not the cauterant produce a further effect, which renders the closure of the vessel permanent; this is the inflammatory action which takes place in the textures contiguous to the cauterised portion, and which, leading to the effusion of fibrinous matter, completes the cure by permanently shutting up the vessel,—and thus, when the slough separates, there is left merely an ordinary suppurating sore, and no further bleeding occurs. The whole point, then, on which the efficiency of the cautery or caustic, or indeed any of the other means of arresting bleeding depends, is, whether the temporary occlusion of the divided vessels will be able to hold its ground till they have been permanently closed by vital action? Now, in regard to the larger arteries, it is found that this is not the case; the coagula of blood formed, and the corrugation of the orifice of the vessel caused, by the chemical agent, are not sufficient to resist the impulse of the blood; and, therefore, even were there not other objections to its employment, the cauterant would be deservedly rejected, on the score of inefficiency, as a general means of arresting bleeding from large vessels. I presume that few of you need to be told, that for this purpose the ligature is the truly efficient and trustworthy resource of the surgeon, and that in all cases where a vessel can be laid hold of and tied, this is the course to be pursued. When this cannot be done—as, for instance, where the bleeding vessel cannot be brought into view, or where the blood is escaping, not from one or two large, but from a multitude of small vessels—pressure is had recourse to; and when we can employ neither pressure nor ligature, we then may have recourse to certain applications, which will produce temporary arrestment of the bleeding, and keep it in abeyance till permanent closure of the vessels occurs. Such applications are called styptics. Some of these, such as cold, act vitally, by producing a constriction of the vessels; some, as alum and other astringents, act partly vitally, and partly, probably, by chemically coagulating the escaping blood, and in slight forms of bleeding this means will often succeed; but when we are deprived of the use both of ligature and pressure, and when the hemorrhage is too active to be controlled by the ordinary styptics, we then have recourse to the more energetic action of the cautery or caustic, as described above.

It is not requisite for me to specify particular cases to which this is applicable—an example will suffice. Take the instance of leech-bites on a child's throat. Here we have no large vessel bleeding, but a number of small open orifices; the ligature is not applicable in the ordinary way,—the situation of the bleeding wounds, of course, prevents our making anything like efficient pressure; we may try cold and other astringents, and in the very vascular skin of a child will probably find them to fail; and we then have recourse to our caustic, which will in general be found to succeed. Or take the instance of hemorrhage from the jaw, after the extraction of a tooth. There is probably a pretty active vessel bleeding, but, being at the bottom of a narrow cavity in a bone, it cannot be laid hold of and tied; pressure may be tried, but the patient will not suffer it to be maintained, from the pain which it occasions in the lacerated gum; we may then have recourse to our caustics, and a liquid one, such as the acid nitrate of mercury, will be advantageous, as it will more readily penetrate to the bottom of the cavity; and, should this fail, we may then employ the actual cautery, using for the present case such an instrument as a red-hot wire.

In using cauterants for the arrestment of hemorrhage, one or two particulars require to be attended to. All clots should be removed from the wound,—it should be cleared of blood by a few moments' pressure with a bit of dry lint,—and the cauterant immediately applied. By this means we secure the advantage of exposing as much as possible the very point from which the blood flows, and thus we can apply the cauterant with more precision; and, by having the wound free of blood at the moment of its introduction, we do not expend its chemical action on escaped blood, but carry it at once and directly to the bleeding vessels.

In cauterising to arrest bleeding from a cavity, as a leech-bite or the alveolus of the jaw, it is obviously essential that the caustic or cautery penetrate to the very bottom of the wound: for the chief source of the hemorrhage is most probably in this quarter; you ought, therefore, to be sure, before applying your caustic or cautery, that it can reach the very bottom of the cavity. I have, in the case of leech-bites in children, seen a piece of caustic applied to the wound, cause much pain, and yet leave the bleeding as active as ever, simply because the piece of caustic was too thick to reach the bottom. Though it is a matter of individual detail, I may mention that, in cauterising leech-bites, I have often had recourse to a simple and ingenious way of making a fine piece of caustic, which was recommended by my late friend Dr. James Hunter,—viz., to break off a piece of nitrate of silver from a common stick of caustic, crush it to powder, heat the point of a silver probe in a candle, and dip it into the powder; it thus becomes crusted with the nitrate, and forms a fine button of caustic, which will easily reach the bottom of any such wound. Lastly, when the bleeding has been stopped, beware of injudiciously meddling with the part. Remember that the occlusion of the divided vessels is as yet only temporary, and may be easily overcome, till the vital processes have been perfected which make the vessels permanently secure. Therefore, let the part be touched as little as possible, and wait patiently for the spontaneous separation of the slight slough which is formed.

Such are the uses and applications of the cautery and caustics, which are founded upon their mere chemical and destructive effect. We saw that the second stage in their action is the production of an ulcer with suppurative discharge. This, as a matter of course, follows the application of cauterants for any object; but sometimes we make use of them to obtain this very result for certain purposes. This is when we wish to employ them as counter-irritant agents, and when we desire to keep up the counter-irritation by the establishment of an issue. As we shall, however, have to consider the whole subject of counter-irritants hereafter, I reserve for a future occasion what I have to say respecting the use of cauterants for this purpose.

There remains, then, only one more of the effects of cauterants which has been turned to useful purposes, viz., their power of forming a contractile cicatrix, and on this point a very few words will suffice.

This effect of a burn, which in cases of accidental injury, often results in very serious deformity, has been applied by surgeons to procure a closure of preternatural openings, which, having become callous on the edges, have lost all tendency to contract naturally, such as vesico-vaginal fistula, preternatural anus, venereal ulcers which have perforated the soft palate, &c. The object which is here in view is to produce a slight artificial burn on the edges of the opening, to allow this to heal, and to gain the advantage, which the contracting tendency of the cicatrix affords, for diminishing the size of the aperture.

It is obvious that to do this successfully, the cauterant must not be applied to such an extent as to produce destruction of tissue, because, if a considerable portion of the edges of the opening were burned away, the sore, when it did cicatrise, would have to compensate, by its contraction, for this loss of tissue, and would not have improved the original malady, nay, might rather aggravate it. In attempting, therefore, to procure the closure of such openings by cauterisation, all that is to be done is to produce enough of inflammation of the edges to cause them to become raw and sore. This inflammation, which is accompanied by some swelling of the parts, often produces at first an apparent direct amendment—the passage of the urine into the vagina in one case, and the escape of fæces in the other, being lessened for a while. But the surgeon knows that this is only temporary; after a day, or even in a few hours, this inflammatory swelling subsides, the escape of fluid is as great as before, and the patient is disappointed. But the surgeon knows that it is not till after this that he is to look for the expected benefit. The sore made by the cauterant is allowed to heal, which it will take some time to do, and now it manifests a tendency to contract, and the orifice, though not closed, is lessened. A cure is not to be obtained, and must not be looked for, from one cauterisation; the same process must be repeated again and again; but this must not be done too soon. An interval of some weeks—sometimes even of some months—should be allowed to elapse before another cauterisation is practised; for, if it be repeated too soon, we interfere with that very part of the process on which the obtaining of a cure depends. It is quite a pathological mistake to suppose that the contraction takes place only when the sore is granulating. From the observation of accidental burns it is quite established that the tendency to contraction continues for some time after the sore of a burn is quite cicatrised. The necessity, therefore, for leaving a sufficient interval between one cauterisation and another is obvious, if we wish to avoid undoing by the second what has been accomplished by the first, and the precise time which is to be allowed must depend upon the extent of the disease and the effect of the first cauterisation. By thus making a succession of slight burns of the edges of the opening, each followed by a certain amount of contraction, we may ultimately close the orifice.

Both caustics and the actual cautery have been used for this purpose, some surgeons preferring the cautery as more decided in its effects, others the nitrate of silver as more manageable. The truth I believe to be that either will answer if the amount of cauterization be duly regulated.

It is right to say, however, that this proceeding is hardly applicable to openings of large size, and that in such cases, if a cure can be achieved at all, it is most likely to be obtained by paring the edges with the knife, and bringing them together by suture. Sometimes by such an operation a great part of the orifice may be closed, but one small portion refuses to unite; and in such circumstances the cauterant method may be applied to this part to complete the cure —*Monthly Journal of Med. Science, Nov.* 1850, *p.* 451.

160.—*On the Nature and Cause of Cholera.* By DR. R. D. THOMSON.—[The following are the conclusions which Dr. Thomson arrives at, after a lengthened and elaborate chemical research into the nature and cause of this mysterious disease.]

1. That the incipient stage of cholera does not differ materially from the common forms of diarrhœa, inasmuch as its treatment is successfully managed by similar means; and this result may lead to the inquiry,—Does not the removal of the symptoms of the disease by narcotics, and therefore, the retention of the fluids in the system, afford an argument against the idea of a morbid poison being the cause of cholera?

2. That in the second stage of cholera, a lymphatic fluid is diffused from the blood into the intestinal canal, corresponding exactly in chemical composition with that secreted or diffused through the serous membranes in hydrocele and hydrocephalus, and other forms of dropsy, Compared with healthy blood, it appears, that the salt which is diffused most largely into the intestines, is common salt, while the albumen of the blood possesses this power of transference generally in a very limited degree. The facts seem to show, that in this stage, instead of as in the natural state, the diffusive power of the mucous membrane being exerted from the intestines towards the blood, the reverse action occurs; thus pointing to a parallelism with purely physical phenomena. Conjoined with other characters, they supply an argument for the inquiry -May not cholera be an *epidemic intestinal catarrh*, influenza being an *epidemic respiratory catarrh?*

3. In the third stage the lymphatic fluid ceases to be poured out from the blood. The bile is excreted, and the normal diffusion from the intestines to the blood resumes its action.

4. There is no evidence of the existence of any organic body in the atmosphere during the prevalence of cholera, and hence the inquiry is suggested —May not this and parallel diseases which are not contagious, such as ague, be principally due to meteorological and physical influences, acting on debilitated habits, and thus a distinction be established between them and contagious affections produced by morbid poisons, as typified by small-pox?—*Med.-Chirurg. Transactions, Vol.* XXXIII., *p.* 97.

161.—CASE OF DISEASE OF THE BRAIN.

By Dr. R. B. Todd, F.R.S., &c.

(Reported by H. H. Salter, B.A., Dem. Anat. K.C.L.)

[William Ware, a ploughman from Kent, was thirty years of age, and of temperate habits. About fifteen weeks before his admission, he was suddenly seized with paralysis of the right side of his body and face. He seems, after this, to have lain in a heavy sort of stupor for some weeks. After the recovery of his consciousness, he continued for three weeks in a state of complete paralysis of the right side. On admission, however, he had somewhat recovered from the paralysis, and he could use the right leg very well, and the arm slightly. Dr. Todd thinks in this case, as the affection was chiefly motor, we may guess at its locality. He thinks it would be in the left corpus striatum, or in that part of the hemisphere which is most intimately connected with it, as this portion seems most intimately connected with the motor function. As to the nature of the affection, it might have been from a clot of blood, from a ruptured blood-vessel, or from a more slow disease of such a nature as to be capable of producing sudden paralysis—as a gradual softening of the substance of the brain. Dr. Todd inclines to believe that this last is almost always the precursor of apoplexy. The artery or arteries leading to the parts are diseased; that portion of the brain fails in its nutrition; passes into the state of white softening; the minute vessels give way, and allow the blood to escape into the tissue of the brain. Another way in which paralysis may take place is by an inflammatory state of the brain substance, producing softening of a different kind to that mentioned, and in which there may or may not be rupture of fibres; but there may be deposit as the result of the inflammation, which, by its pressure, may interfere with the conducting power of the fibres. Dr. Todd proceeds:]

Now I have mentioned the opposite conditions of *white* and *red* softening. These are apt to come on under opposite circumstances. *White* softening may be caused by anything that diminishes or cuts off the supply of blood to the head. In some cases, in which the carotid artery has been tied for aneurism, and the supply of blood to the brain on that side thus stopped, the patient may go on very well for a short time, and there may be no impairment of function; but during that time the process of softening has been gradually going on on that side of the brain which is supplied by that carotid; and in a day or two the softened fibres cease to maintain their continuity, and give way, and paralysis ensues.

Some years ago I attended, along with my friend Mr. Street, of Norwood, a very remarkable case of this sort. The supply of blood had been cut off by a dissecting aneurism, which had plugged up the common carotid artery on the right side, and paralysis of the left side of the body took place. We were much puzzled to account for the paralysis till after death, when the post-mortem examination cleared it up. The account of this case was published in one of the volumes of the 'Medico-Chirurgical Transactions.'

There is abundant evidence to show that under ordinary circumstances white softening is *atrophic* —i. e., dependent on imperfect nourishment of the brain, and non-inflammatory. But it may exist on the confines of genuine inflammation, the nutrient fluid being diverted from it to the inflamed portion, or it may exist around an effusion of liquid in the ventricles, that effusion being possibly inflammatory in its nature.

The second sort, the *red* softening, is inflammatory. I show you here a very good representation of it, which was made from a case of mine some time since. The portion of the brain affected was of considerable extent; there was paralysis, of course, on the opposite side; there was a stupid, comatose condition, not complete coma, and loss of sensibility, which continued till the patient died. But the paralysis came on gradually, and not till after these comatose symptoms had existed some time.

Now of these two conditions I think we may very justly attribute the symptoms in our patient Ware's case to a white softening, followed by a rupture of blood-vessels, and an effusion of blood.

The numbness, which he described as having occurred before the paralysis, indicated that morbid changes were slowly creeping on, and after a time rupture of the blood-vessels, and disruption of fibres, took place, when followed the paralysis and the three weeks' stupor. The former being very complete, denoted a very complete solution of continuity on some part of the centre of volition; the sudden stupor implied that pressure existed within the cranium, but only to a moderate extent, for a large effusion, capable of producing such a complete paralysis, would undoubtedly have caused profound coma.

This, I say, seems the *most probable* train of morbid processes, at the commencement of the case; but we cannot speak on this subject with great certainty, for we cannot fully depend on the accuracy of the patient's history of himself. It tallies, however, very well with what we observed of his subsequent history, and of the post-mortem examination.

A train of phenomena, however, very similar, but differing as regards the *sudden* supervention of the paralytic state, might have occurred, when the primary lesion was of the inflammatory kind.

Upon examination of the brain after death, it was found that a considerable portion of the corpus striatum of the left side was completely destroyed and excavated, and that the cavity was filled with a creamy fluid, having somewhat the appearance of pus; the anterior and inner part of the corpus striatum was healthy, but the whole of the posterior and outer part was thus disorganized; a few bands of fibres, easily broken down, passed from one side of the cavity to the other. The creamy matter contained in the cavity was found, on microscopical examination, to contain great numbers of large cells, containing oily matter in large globules, and also in a state of extremely minute subdivision. These curious organic globules might suggest the idea, that some active reparative process was going on during life. What their precise signification is, I do not pretend to determine; but I think I may affirm that they are characteristic of a state of white softening, as I have found them in other cases, in which no doubt could exist of the atrophic nature of the lesion.

It seems almost certain, then, that this excavation of a portion of the corpus striatum must have been due to a white softening, followed by an effusion of blood, and which was the immediate cause of the breaking down of the fibres, and of the pressure which gave rise to the paralysis. No traces of blood remained on the diseased part, as no doubt there had been ample time for the disintegration of its particles, and for its absorption.

At first the paralysis was very complete, but afterwards the patient recovered a certain amount of power, especially in the lower extremities. If the paralysis had been accompanied with rigidity, I should have been led to the conclusion that the cerebral lesion was of an irritating nature. This rigid state of the paralysed limb *when it comes on at the same time as, or very soon after, the paralysis*) is generally seen when some superficial part is affected, as the meninges, or the surface of the brain, or when there is a growth from the skull, or a tumour in the hemispheres, in some cases of inflammatory softening, or in some conditions keeping up a constant irritation; but when there is simple rupture of the fibres of a deep-seated part of the brain, as the corpus striatum, with or without pressure, there is no irritation: the paralysed muscles are quite lax.

There was no appearance whatever of rigidity in the muscles of the paralysed limb. Now this is just the sort of paralysis (accompanied as it was by comatose symptoms) which would arise from a solution of continuity of fibres in the first instance, and from compression. A portion of the corpus striatum is destroyed, and the healthy part is compressed by the effused blood. On the absorption of the latter a certain amount of power had returned in the limbs; and it depended on the possibility of repairing the broken-down portion, whether a complete restoration would take place. That parts so much diseased would have ever been completely repaired seems extremely unlikely for two reasons—first because the arteries of the part did not seem in a perfectly healthy condition; and, secondly, because of the well-known tardiness of all reparative processes in the brain, where actual solution of continuity has taken place.

You remember that on several occasions we passed the galvanic current through the paralytic and the sound limbs in this case. This was done, as I explained to you at the time, for the purpose of ascertaining whether any irritant disease existed within the cranium at the seat of the paralysing lesion.

If, on passing the galvanic current, you produce less contraction in the paralysed than in the sound side, then you may judge the cause to be of a *depressing* kind: if, on the other hand, the contractions in the diseased limb are the strongest, then you may conclude that the condition of the centre which causes the paralysis is *irritative*. But then you may bear in mind that that irritation is not always inflammatory.

The most important points in this case may be thus summed up:—

First, through some diseased state of the assimilative process, the arteries of the brain become diseased, and an insidious gradual process of softening takes place; rupture of one or more blood-vessels follows upon this, with solution of continuity of fibres, and compression of the neighbouring healthy brain structure; after this we have absorption of the clot, and more or less attempt at reparation: it may be that some inflammation may take place around the clots, which may retard the process of recovery.

If you were called upon to treat a case of this kind from the beginning, what course would you adopt? If you can clearly make out that the lesion is not inflammatory, but, on the contrary, due to defective nutrition, the less you interfere the better. Keep the patient in the horizontal position, with the head a little raised; let the head be kept cool; unload the bowels in such a way as will involve the least effort on the part of the patient; a large turpentine and castor-oil clyster is generally more expeditious than purgatives given by the mouth. Bleeding is generally not admissible in such cases as this, as it tends to increase the atrophic condition of the brain, and would under such circumstances, favour rather than repress hemorrhage.

When this patient came under treatment in the hospital, we did not at first administer any medicinal agent: he was kept quiet and nourished moderately. After one or two trials with the galvanism we observed, on one occasion, a little more excitability in the palsied limbs than in the sound, under the influence of the inverse current. Finding that he had made no progress for some days, I was led, from this effect of the galvanism, to fear that some inflammation might have set up around the lesion; and accordingly, I was induced to give him drachm doses of the solution of the bichloride of mercury—that is, as you know, the sixteenth of a grain of that salt three times a day. No good effect followed this treatment: on the contrary, I fear the salivation which took place sooner than might have been expected must have made him more susceptible of the poison of erysipelas.

After all, I must acknowledge that in this part of the treatment somewhat of the *nimia medici diligentia* was exhibited. It is a lesson hard to learn, and more difficult to act upon, that nature can do more than the physician; but it is a lesson which each succeeding year of increasing experience will impress upon you, and in no cases more than in those of chronic affections of the brain.—*Med. Gazette, Nov.* 1, 1850, *p.* 735.

162.—CASE OF DISEASED BRAIN CAUSING DOUBLE APOPLEXY.

By Dr. R. B. Todd, F.R.S., &c.

(Reported by H. H. Salter, Esq., B.A.)

[The following case seems to illustrate the mode in which white softening of the brain is apt to take place, and the way in which that disease favours the production of apoplexy. The subject of it, George Regan, æt. 59, admitted he had been in the habit of drinking a good deal of beer and spirits. He had suffered, also, for a long time from an affection having the nature of gout.]

About two years ago he was seized suddenly while at work, at nine o'clock in the morning, with a feeling of stupor: as he happened to be working at home at the time, he laid down on his bed, hoping to sleep it off by dinner-time, but when he attempted to get up he fell, and found that he had lost the use of his right side; he then became comatose, and so continued for some days. Whilst in this state of insensibility he was taken to a neighbouring hospital, where he remained for two or three months, and at the end of that time in some degree recovered from his attack, but not so as to enable him to work as he had done before his illness.

During last October he had a *second* attack, of the same kind as the first, but less severe; the limbs were paralysed, as before, on the right side; he remained a few days in bed, and recovered in some degree the use of the arm and leg, but he has never been able to work since.

On Christmas day last, at nine o'clock in the morning, before he was up, he had a *third* attack; on this occasion he was delirious for a short time, and afterwards became insensible, but he recovered his consciousness in the evening.

This third attack brought on increase of the paralysis on the right side, so that on this occasion not only was the motor power affected, but the sensibility likewise. This latter function was so far affected, that he could not pick up small objects, and he would frequently let fall things which he meant to retain in his hand, and he staggered when he attempted to walk. It was more than three weeks after the attack when he was admitted, and the paralytic state had not improved; on the contrary, he thought it worse. His condition, on admission, was as follows: there was imperfect palsy of the right side, as shown by slight ptosis of the right upper eye-lid, and a slight hanging of the cheek, the features being slightly drawn to the *left* side. He would protrude his tongue straight, but his articulation seemed somewhat difficult. In walking he slightly dragged the right leg; the grasp of the right hand was moderately firm, but not *so* firm as that of the left; the muscles of the right leg and arm were flaccid, and less nourished than on the left side. The impulse of the heart was very strong, and a mitral systolic bellows sound could be distinctly heard.

For some days after his admission he suffered very much from constipated bowels, which it required the strongest purgatives to overcome.

He then began to complain of heaviness of the head, a disposition in his thoughts to wander, and a difficulty in collecting them. These symptoms appeared to me to portend the approach of another attack, such as he had on three former occasions. His remarkably sallow complexion and general leucophlegmatic appearance led me to suspect the existence of renal disease, resulting from that particular form of kidney —small and atrophied—which so frequently accompanies a gouty condition. This opinion was confirmed by the characters of the urine, which was pale, of low specific gravity, and slightly but distinctly albuminous. With this view, and imagining that the uneliminated urea might be contaminating the blood, and affecting the brain, I ordered him to be freely blistered at the back of the neck. This seems to have somewhat relieved him, for the next day he was reported to feel rather easier as to his head, and to have more power over his thoughts. At nine o'clock, however, on the following morning, the house-physician, Mr. Armitage, was called to him in consequence of his having been suddenly seized with a fit; he found him completely paralysed on the *left* side, both as to sensibility and voluntary motion; his left eye squinted, and was twisted downwards and inwards; at every respiration his left cheek puffed out from want of power of the buccinator; the right leg and arm moved when pricked, the left not; his breathing became louder and more sterterous, his coma deeper, and at last he died.

There was in the attacks to which this patient was subject a curious combination of the epileptic and the apoplectic, the one following upon the other. At the foundation of them, no doubt, was the diseased state of kidney. The first attack of sudden stupor was probably a slight epileptic seizure, the effect of which was a disturbance in the circulation in the brain, and the giving way of some small vessels—a slight apoplexy, with compression and rupture of fibres, and consequent paralysis.

Whilst the one or two small clots which had been effused on this occasion were undergoing absorption, and some attempt at reparation was taking place, he had another seizure of the epileptic kind,—a further disturbance of the circulation and nutrition of the brain on the same side, probably in the corpus striatum, or the fibres which pass from it to the hemisphere. But as the paralytic state appears to have been only slightly increased, and as the coma was of short duration, it is not likely that any effusion of blood took place on this occasion.

The epileptic character of the third attack was manifested in the delirium with which it was ushered in, and which ended in coma. There was a decidedly increased paralysis after this attack; but as quite as much paralysis is apt to follow the simple epileptic seizure, it by no means follows that any effusion of blood took place, although it is probable that such must have been the case, from the fact that the palsy showed no signs of improvement, as is generally the case with the epileptic palsy. As this attack occurred at a time when Christmas festivities are more or less prevalent, with all ranks and classes, it is very likely that the immediate exciting cause of the attack was due to over indulgence of some kind.

The fourth attack had in its premonitory signs all the characters of a threatening epileptic paroxysm; and knowing, as we did, the existence of renal disease, we were prepared for such an attack. The epileptic coma, however, soon passed into the profounder coma of a compressed brain; and as a new hemiplegic paralysis of a very complete kind showed itself on the left side instead of the right, it was easy to infer that a new and extensive apoplectic effusion must have taken place on the right side of the brain.

Assuming that the first three attacks were epileptic, followed by an apoplectic effusion, and that this effusion was due to a weakened condition of the arterial coats as the result of disease, it was quite consonant with experience to attribute the fourth attack to a similar cause, and to infer that arteries similarly diseased had given way on the left side of the brain. Most cases of rupture of vessels in the brain at the age of this patient take place from disease of the arteries; and it is very common, as was first pointed out by Bizot, for the arteries of the brain to be affected in a symmetrical manner—*i. e.* corresponding arteries of opposite sides will be similarly diseased, and to nearly the same extent. In this way disease had been, for some time, making progress in this man's brain symmetrically; there was first palsy of the right side, and then a similar condition of the left; and this simple fact of symmetry pointed to the arterial system as the seat of disease, and therefore to its usual result, apoplexy.

On opening the patient we found there was a double apoplexy corresponding to the double paralysis—one of long standing, and one recent—the first on the left hand, the other on the right. The original hemorrhage affected the corpus striatum and optic thalamus on the left side; and it was evident that the hemorrhage on this side did not take place at once, but on two occasions at least. The substance of the corpus striatum had evidently been the seat of the older effusion. It exhibited on section several bloody clots, and that peculiar yellow discolouration which always succeeds to a hemorrhage. The more recent effusion was a clot which had formed quite on the surface of the optic thalamus, extending likewise to the corpus striatum, and lodged in a depression on the surface of those bodies. We had thus an explanation of the cause of the original paralysis of the left side; and its imperfect nature was plainly due to the fact, that there was but little destruction of the corpus striatum, and that the compression of the latter clot affected chiefly the optic thalamus, and but slightly the corpus striatum.

But the apoplectic effusion on the right side was much more extensive; it involved parts corresponding to those affected in the first seizures, but to a much greater extent; and the brain substance was more completely torn up and destroyed. The clot was very large, and it entirely filled the right lateral ventricle, breaking through its roof, and tearing up the white substance of the cerebral hemisphere: it likewise tore up the corpus striatum, optic thalamus, and the septum lucidum, all which parts were completely swept away. This remarkable destruction was no doubt owing to a previous diseased state of the brain. I mentioned to you in my last lecture that it is very common for apoplectic effusions to be preceded by white softening; and when the softening has got to a certain extent, the brain substance is no longer an adequate support to the vessels, which, themselves more or less enfeebled by disease, give way, not in one point only, but in many, and the abundant effusion ploughs up the softened matter, quite destroys it, and takes its place: all this was the case in the present instance. The arteries of the brain were very generally diseased on both sides, and exhibited that symmetry to which I have alluded.

It is probable that the first part of our patient's last attack—the disposition of his thoughts to go wool-gathering—took place in connection with the softening; while the final catastrophe—the apoplexy and death—resulted from the hemorrhage to which that softening, coupled with the diseased condition of the arteries, inevitably led.

We found, likewise, as had been anticipated, a very diseased state of the kidneys: they were very contracted, the cortical substance much wasted, fissured, and granulated on its surface, the tubular substance healthy, and the capsule thickened. This condition of kidney, formerly described as the third stage of Bright's disease, is, in reality, a chronic degeneration or wasting of the kidneys, due to a deranged and damaged nutrition, for which I should be glad to find some other name than chronic nephritis: I have called it *gouty kidney;* and in this and many other cases this name is very appropriate. But it occurs in cases where there is no evidence of gout. The result of the disease is to render the kidneys imperfect emunctories for the elimination of the urea, and other elements of the urine, which accumulate in the blood, and give rise to various morbid changes throughout the body, and are especially mischievous to the functions of the brain.

The heart and the arterial system were likewise extensively diseased. In the coats of the arteries were very numerous deposits of atheromatous matter. The heart was much dilated and hypertrophied, especially as regards the left ventricle. The fibrous basis of the valves was extensively thickened, apparently by some deposit, which rendered it opaque, and impaired its flexibility; and, as is generally the case in this particular form of disease, *all* the valves were altered in this way; those of the right side, however, being much less diseased than those of the left. The semilunar valves of the arteries, especially of the aorta, had the fibrous festoons at their bases much thickened, the curtain of the valve being very little affected, and its function, therefore, but little impaired; and the cordæ tendinæ of the mitral valve were much thickened, and somewhat shortened, and the curtains of the valve also thickened. Similar changes had taken place in the tricuspid valve and its tendinous cords, but to a much less extent.

Taking, then, a general retrospect of this case, we find there is quite enough to account for all we have seen. The sequence of the events may be thus described:—First, the man gets into a general gouty condition, and the elimination of this morbid material gives rise to an irritation of the kidney, which at length assumes the form of gouty kidney, or, if you will, chronic nephritis; and this chronic nephritis incapacitating the kidney for the perfect discharge of its function is the cause of all the subsequent maladies: the blood becomes contamined, deposits take place in the tissue of the heart's valves, in the large arteries, and in those of the brain; the diseased arteries of the brain become insufficient channels of supply, white softening is the consequence, and the unsupported and unhealthy arteries at length burst; and thus all the circumstances, from first to last, fall in regular order as cause and effect.

The deposits in the arteries produce a two-fold influence upon the circulation—by roughening the inner surface of the arterial channels they create a certain amount of direct obstacle to the flow of blood from the ventricle; and by diminishing, or nearly destroying, the elasticity of the arterial walls, they destroy one of the most important forces by which the circulation is carried on in the arterial system. Thus the arteries, from being elastic yielding channels, with perfectly smooth inner surfaces, are changed into resisting inert tubes, with rough inner surfaces. It is plain, then, that, under these circumstances, the heart has to encounter great obstacles, and to do a great deal more work than when the arteries are in their normal state. Hence the dilatation caused by the obstacle to the free flow of the blood; and the hypertrophy, by the greater exercise and effort of the muscle of the heart. The increase of force is merely remedial, to meet the increase of obstacle, and is one of those beautiful instances of self-adaptation to change of circumstances with which the animal organism, especially the muscular system, so much abounds.

As these deposits go on they impair the materials of the arteries of the brain; the degenerated walls of these vessels possess less strength, and are less able to support their contents. There is no undue determination of blood to the brain, but the reverse, for the blood that goes to the head has, in the erect posture, to be pumped up against the force of gravity; and, therefore, any obstacle in the course of the arteries would be more felt in this direction than in any other. It is a common notion that the hypertrophy of the heart gives rise to the apoplexy, by sending the blood with an undue impulse to the head; but for the correction of this error we need only remember that the additional force is merely such as is necessary for the exigencies of the circulation, and such as shall preserve the force of the blood's current as near as possible to the normal point, in spite of the existing obstruction. The actual force with which the blood circulates in the morbid arteries is most probably less rather than greater than in health. The apoplexy is, in fact, due to the diseased state of the arteries, which renders their walls an inadequate support to their contents, and to the diseased state of the brain, which imperfectly supports the arteries.

A vast number of the cases of apoplexy which occur about the period of life of our patient, or after the age of 50, are of this kind—a fact that has an obvious and an important bearing upon the question of treatment.

There is a practice, unfortunately too common, but which, I think, is every day becoming less common—namely, that of following an attack of apoplexy by depletive measures, very much as a matter of course. However applicable such a mode of treatment may be to strong, young, hale, and plethoric subjects, I presume no one will say that it is very well adapted to patients who have passed the meridian of life, and whose blood and whose tissues are more or less contaminated by gouty matters, and with whom a morbid state of the arteries of the brain has already greatly weakened the nutrition of that organ. The case, indeed, which I have just detailed to you, is one of many which proclaim loudly that a depletory system ought not to be pursued indiscriminately, or even generally, in apoplectic cases.

With reference to this question of depletion in apoplexy, I would refer you to an interesting and very useful work by Mr. Copeman, in which he has collected, from a great variety of sources, a large number of cases which presented the symptoms of apoplexy. Of 155 cases in which the treatment was specified, 129 were bled, and only 26 were not. Of the 129 who were bled 51 recovered, and 78 died—the cures being 1 in 2½, the deaths 1 in 1⅗. Of the 26 who were not bled 18 were cured, and 8 died, the proportion of cures being 1 in 1½, and of deaths 1 in 3¼ Eighty-five of the cases were bled generally and copiously, and of these only 28 recovered, and 57 died—in other words, two in every three cases terminated fatally. I am quite aware that the small number of cases not bled casts some doubt on the validity of the conclusion to be drawn from the comparison of the results of the treatment. But the fact that considerably more than half of those treated on the antiphlogistic plan died (and we owe much to the industry of Mr. Copeman for bringing it out), is a highly significant one, and should arrest attention.

[Dr. Todd adds, the treatment of patients with apoplectic symptoms should not be regarded as a matter of routine; and that, however much popular opinion may be opposed to it, we must by no means rashly adopt the usual heroic practice of opening a vein upon the spot. Having placed the patient in a proper position, he says, we should direct our attention to the state of the stomach and bowels; and as these are often overloaded, we may clear them out by an active purgative administered by the mouth, or by stimulating and purgative enemata. Dr. Todd concludes:]

On the whole, then, I think that the results of experience denote that the majority of cases of apoplexy are best treated by purging, shaving the head and keeping it cool—perhaps blistering, and that bleeding is rarely applicable, except to the young, vigorous, strong, and plethoric.—*Med. Gazette, Nov.* 8, 1850, *p.* 779.

163.—*On the Structure of the Membrana Tympani in the Human Ear.* By JOSEPH TOYNBEE, Esq., F.R.S.—In this paper, the membrana tympani is described as consisting of the following layers, which are quite distinct from each other, both as regards their structure and functions:—

1. Epidermis.
2. The proper fibrous layer, composed of—
    - *a.* The lamina of radiating fibres.
    - *b.* The lamina of circular fibres.
3. Mucous membrane.

One of the principal objects of the paper is to describe the structure and functions of the fibrous laminæ. Since the time of Sir Edward Home, who pronounced the layer of radiating fibres to be muscular, anatomists have differed in their views of the nature of the fibrous element of the membrani tympani. The lamina of radiating fibres, the outer surface of which is covered by the epidermis, is described as continuous with the periosteum of the external meatus. With the exception of the uppermost fibres, which, on account of their being somewhat flaccid, have been considered as a separate tissue, under the name of "membrana flaccida," the radiate layer is composed of fibres, which extend from the circular cartilaginous ring to the malleus, and they interlace in their course. These fibres are from 4000 to 5000 parts of an inch in breadth.

The lamina of circular fibres consists of circular fibres, which are firm and strong towards the circumference, but very attenuated towards the centre. These fibres are so attached and arranged as to form a layer of membrane, which, in a quiescent state, is saucer-shaped.

The fibres composing the circular, are smaller than those of the radiate lamina, being from 6000 to 10,000 parts of an inch in breadth.

The facts that appear to be adverse to the idea of the fibres of either layer being muscular, are—

1st. The absence of distinct nuclei in the fibres.

2nd. Their great denseness and hardness.

It is next shown that the four laminæ forming the membrana tympani are continuous with other structures, of which they appear to be mere modifications, and that not one is proper to the organ.

The tensor tympani ligament, which had not been previously noticed by anatomists, is particularly described; it is attached externally to the malleus close to the insertion of the tensor tympani muscle, and internally to the cochleariform process.

The latter part of the paper is occupied by observations on the functions of the fibrous laminæ, and of the tensor ligament of the membrana tympani, and it is shown that by these two antagonistic forces, the one tending to draw the membrana tympani inwards, the other outwards, this organ is maintained in a state of moderate tension, and is always in a condition to receive ordinary sonorous undulations. –*Lancet, July* 27, 1850, *p.* 181.

164.—*On a New Speculum Auris.* By J. Cooper Forster, Esq., M.B.—[Although Mr. Forster considers the small conical tube recommended by Mr. Wilde, of Dublin, a most useful instrument in ordinary cases as a speculum auris, yet, when the canal is deep, sufficient light cannot be thrown by it on the membrana tympani; and to obviate this inconvenience he has added to Mr. Wilde's instrument three-eighths to half an inch of silver tubing, of the diameter of the small end of the cone, by which, he says, the membrane is more clearly seen, and any changes in the deeper portion of the canal more easily recognised.]—*Lancet, Aug.* 3, 1850, *p.* 148.

165.—*On the Diffuse Opacity of the Cornea resulting from Corneitis, treated by Stimulants.* By Dr. Jacob.—From the variety of stimulants used from time immemorial to remove opacities of the cornea, and the number of them extolled as infallible, it may be presumed that any stimulant will answer the purpose. Solutions of nitrate of silver, sulphate of copper, sulphate of zinc, or the combination called *lapis divinus*, will perhaps answer. I use a solution of iodide of potassium, ten grains to the ounce of water; or, as a substitute for animal bile, said to be effectual, touch the surface with the camel-hair pencil previously dipped in water and brushed two or three times on soap. The fumes of prussic acid, so much vaunted as a quack remedy, I have not used, being dangerous and troublesome. If this nostrum has any influence at all, it is as any other stimulant.—*Brit. and For. Med. Chirurg. Review, July,* 1850, *p.* 106.

166—*Muscæ Volitantes.*—Brewster has founded, on experiments with his own eye, a view respecting the *muscæ volilantes,* which differs from the explanations given by De la Hire, Porterfield, and Mackenzie. He points out as the cause, portions of the cells in which the vitreous humour is inclosed, of which the torn filaments float about in the chamber, and throw shadow and reflected light on the retina. By means of two bright lights placed before the eye, two shadows of the same fibre were obtained, by the aid of which Brewster has measured the apparent diameter of the *muscæ volitantes,* as well as the distance from the retina of the bodies producing it. According to Brewster, the muscæ occur in every healthy eye, becoming dangerous only by too great an accumulation; they are, therefore, by no means a symptom of approaching blindness from cataract or amaurosis. —*Phil. Mag.—Brit. and For. Med.-Chirurg. Review, July,* 1850, *p.* 250.

167.—*Case of Entropium and Trichiasis.* By H. HAYNES WALTON, Esq., F.R.S.—[This was the case of a female, æt. 40, who had had these diseases three years. Some of the ciliæ had been removed; but as her state was becoming more distressing, she applied to the hospital. Mr. Walton says:]

I operated for the entropium, and removed rather more skin than was sufficient to restore the lids to their natural position, and everted them a little, to get the lashes out of the reach of the cornea. In the right eye the intention was answered, but, in the left, a bundle of cilia, that passed from the centre of the lid, still encroached on the globe. Some weeks after, I removed them in this manner: I placed the tip of the fore-finger under the lid, to raise it and render it firm, and also to protect the globe; then I made three incisions through the skin and orbicular muscle, down to the cartilage, one on each side of the cilia and a third above them, and seizing the little trilateral piece dissected it off, taking care to keep the edge of the knife close to the cartilage that the roots of the cilia should not be cut across, for, unless they are entirely removed, the operation would not be effectual. The operation may be done in other ways,—and I will show how when I come to speak of trichiasis alone. After the incisions have been made it is not absolutely necessary that the lid be supported by anything placed under it, but perhaps it is better that it should be, and, as the fore-finger is required for the use of the forceps, the third finger must take its place.—*Med. Times, August* 10, 1850, *p.* 137.

168.—ON THE MEDICAL JURISPRUDENCE OF INSANITY.

By Dr. ROBERT JAMESON.

A careful distinction, says Dr. Jameson, must be made between *diseases having mental symptoms,* and diseases of the mind; betwixt madness, and such affections as hypochondriasis and hysteria. Hypochondriasis consists of illusions and hallucinations regarding one's bodily sensations, and is therefore not insanity, until these become deep-seated and permanent delusions, compelling to irrational conduct. Hysteria, hypochondriasis, and various other affections, which often precede, cause, and accompany insanity, are not diseases of the mind, but more strictly speaking, diseases having mental symptoms, until by generating delusions they pass into insanity, and afterwards continue as complications of the disorder which they have created. Thus we have hysterical mania, which is not hysteria merely, but hysteria and mania combined; so also there is hypochondriacal monomania, which is not hypochondriasis alone, but hypochondriasis and insanity together.

Simple defect of intellectual power, *intellectual dulness*, must not be confounded with the imbecility which constitutes a variety of insanity, which is not merely a feeble intensity of mind, but a positive deficiency of faculties. Wherever there is a feeble exercise of attention, or defective memory, there will be imperfect comparison, and resulting inaccuracy of judgment; but not from these, or similar causes, any necessary tendency to be governed by delusions. In all cases of imbecility included in the term unsoundness of mind, there are both emotional and intellectual deficiency, a loss of control over conduct, and from the antecedence of judgment being in some part defective or diseased, a liability to confound the actual with the unreal. Delusion is not a prominent characteristic, however, for in such cases feebleness is the type of all the mental manifestations, whether healthy or disordered.

Eccentricity has also to be distinguished from insanity. All the insane are eccentric in their ideas, their language, or their conduct, but the merely eccentric have but a voluntary resemblance to the insane. Eccentricities differ from lunacies in not arising from a loss, but from an undue exercise, of the faculty of judgment; very often from a vanity or self-respect in the individual, that leads him to prefer his own judgment to all other judgment, experience, and authority. They offend against custom and experience more than against reason. The eccentric, if he cannot give a satisfactory reason for his *outré* conduct, can at least assign an intelligible motive: the lunatic has no explanation to afford that does not involve an absurdity. A certain individual behaves in all respects like other men, unless that he constantly walks about without a hat, or any other artificial covering for his head. When questioned upon this point of disagreement with the custom of his neighbours, he says, that nature did not intend the head to have any other protection than what she herself had afforded, that a bare head is more becoming, that he feels himself in every way more comfortable as he is, and that he is certain that he will live longer in consequence of acting in this rational way. In two instances in which lunatics adopted the same habit, the causes assigned by them were of a very different description. One did not have his head covered, because it had grown so large that he could not get a cap to fit it; and the other was so annoyed by certain mischievous tormentors of an invisible kind drumming upon the crown of his hat, that in general he preferred to carry it in his hand. The eccentric man had a rule of conduct, the result of his own narrow judgment; the two lunatics were impelled by fancies upon which their judgment was entirely inoperative. Lord Monboddo, insisting that the human family were originally adorned with tails, showed himself an eccentric theorist: had he asserted that they actually retained them, he would have had an insane delusion, instead of a philosophic crotchet. He would have had a false perception on which his judgment was inoperative, whereas he was guided *by his judgment* to a strange conclusion.

Eccentricities no more constitute insanity, than idiosyncracies constitute disease; and as these are competent with a sound state of body, so are those with even a vigorous judgment. For example:—there was an old man well known in London in the last century, who was of an ungainly appearance, and subject to occasional attacks of hereditary melancholy. So inconsistent was he in his habits, that sometimes he practised great abstemiousness, and at other times devoured huge meals, with brutish slovenliness and voracity; sometimes he would persist in drinking nothing stronger than water, but occasionally he drank wine by tumblerfuls. His income was far from large, and not of a certain amount, yet he kept a set of old men and women about his house, whose bickerings and disagreements now and then drove him out of doors. He was in general very loquacious, but had been known to sit in company and drink a dozen cups of tea, without speaking a syllable. When not engaged discoursing, it was his custom to keep muttering to himself. In walking he performed strange gesticulations with his limbs, and would not go in at a door, unless he could effect his entry in a certain preconceived number of steps, and so as to introduce himself on a particular foot, turning back, and recommencing, until he succeeded as he desired. There was a row of posts near his house which he would not pass without touching singly, and if he found that he had omitted one in the series, he retraced his steps to remedy the neglect. He hoarded up orange-skins for some mysterious purpose which he would never divulge. He suffered remorse of conscience for once having taken milk with his coffee on Good Friday. He believed in ghosts, and went ghost-hunting in Cock-lane; and he maintained that he had heard his mother calling upon him by name from the other world. Yet Dr. Johnson was so far from insane, that his judgment commanded respect and admiration everywhere, and by the common consent of eminent contemporaries he was the most vigorous thinker and the greatest sage of his time.

There are, however, instances of eccentric conduct resulting from *abuse of the imagination*, which verge closely upon madness, and occasionally pass into it. The distinction is, that the individual's voluntary power over his thoughts is capable of bringing the comparing faculty into efficient operation, when he chooses duly to exert his will. Thomas Hood speaks of one who, in consequence of exciting his fancy by German tales of diablerie, used to fly up stairs at his utmost speed from the street door to the attics, because a sort of wager with the devil came into his head, that he would gain the top before counting a certain number, or forfeit eternal happiness. Every one possibly experiences moods which differ in no respect from insanity, than that they are neither permanent nor independent of the will. These may be common in imaginative minds, but instead of being indulged, they should be guarded against and restrained, for, though they exist at first by sufferance of the will, they sometimes gain a strength that defies control, and triumphs over the reason. Hoffman, a master in fantastic fiction, suffered so much from intemperate abuse of the imaginative faculty, that solitude became terrible to him. He was never quit of a mysterious sense of danger; things the most cheerful became incongruously associated with thoughts the most dreadful, while monsters and spectres, which he himself had created, tyrannized over his reason. To appease his terrors, he had frequently to summon his wife from bed, to sit by him as he studied at night. He was constantly on the verge of insanity, and died of spinal disease, his mind being tortured by his fancy to the last hour of his existence.

Often it is not an easy matter to draw a distinction between insanity and *moral depravity*. In every case of mental alienation there is disorder of moral as well as of intellectual manifestations. The moral disturbance is usually the earliest developed feature of the malady, frequently it is the most prominent, and occasionally no other is distinguished; so that the bad have been liable to pass as mad, and the mad for bad, according to the philosophy and fashion of the time. There is no problem in law, medicine, and ethics, of greater social importance, or of more difficult solution, than the discrimination of insanity and vice. A wicked deed may be the result of an undeveloped moral sense, as for example in a child or an idiot; it may be the consequence of such extremity of passion as impels to instinctive, instead of deliberative action; and it may be the indication of a conscience enfeebled by voluntary neglect, and the habitual gratification of evil desires. Any of these states may be simulated by disease; but it is with the two last only that the jurist will have difficulty; for in all cases of insanity in which the moral sense is non-existent, the powers of the understanding also are either undeveloped or destroyed. No hideousness of depravity can amount to proof of insanity, unsupported by evidence of a judgment incapacitated, or a will fettered, by disease. In those cases of mental disorder in which the emotions are perverted, and where there is no clear proof of deranged intellect, cases which do from time to time occur, the presumption of insanity, in regard to a criminal action, has to be upheld by evidence of suspension of the will. The actions of an individual in such a state ought to be impulsive, involuntary, and irreconcileable with the idea of a healthy state of the emotional faculties.—*Medical Gazette, Aug.* 2, 1850, *p.* 179.

A SYNOPSIS,

CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOREGOING PAGES OF THIS VOLUME; AND SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

DISEASES AFFECTING THE SYSTEM GENERALLY.

CANCER.—Dr. Bennett has remarked that "in a cancerous growth, the tendency is to excessive cell-formation. We evidently retard its advancement by the application of cold. Were it possible, indeed, to bring down the temperature of an entire growth below the vegetating point, we must inevitably kill it; but supplied as it is through the warm blood within, this is impossible." The practice of congealing the parts as a cure for cancer has been attended with great success. Five minutes is the usual time to apply the congealing agent to the exterior of the body, when the skin and subjacent tissues are in their normal state; but it may be applied for fifteen or twenty minutes in the case of uterine cancer, and probably not with half the effect as when it is applied to the exterior, on account of the higher vascularity and natural heat of the part subjected to it. Congelation may be locally effected by mixing quickly half a pound of ice with half that quantity of common salt. This may be poured into a net of the thinnest silk gauze, and immediately applied to the part. The brine, as it trickles from the net, may be absorbed by a moist sponge." (Dr. J. Arnott, p. 16.)

[For a peculiar apparatus for applying this mixture to the os and cervix uteri, see p. 17.]

FEVER.—*Hay Fever.*—Let ten drops of the tincture of the nux vomica of the Dublin Pharmacopœia be taken for a dose, in water, and increased gradually to twenty drops, three times a day. It is an agreeable light bitter, increases the appetite, and influences the Schneiderian membrane, no doubt through the nerves. An ointment may also be applied as high up in the nostrils as possible, composed of ʒ iss. of Goulard's extract, ℥ij. spermaceti cerate, and a few drops of oil of roses, or of bergamot. (Mr. G. T. Gream, p. 7.)

Intermittent.—M. Baud has discovered a new salt "hydroferro-cyanide of potash and urea" (!) which is stated to be far more efficacious than quinine in the treatment of all malarious diseases, and that the majority of cases in which quinine and arsenic had failed, were cured by it.—*Med. Times, Aug.* 24, 1850, p. 210.

Rheumatic.—In a case of rheumatic fever in Charing-Cross Hospital, half an ounce of lemon-juice, given three times a day, was found very successful in removing the pains in the joints, after various other remedies had been tried. (Mr. Hancock, p. 29.)

Spotted Fever.—In the bloody diarrhœa of spotted fever, give two or three grain doses of gallic acid. (M. Homburger, p. 427.)

Scrofula.—Preparations of walnut-tree leaves have lately been introduced as a remedy for strumous disorders. The cases which have been treated with them have afforded some astonishing results. Out of fifty-six patients so treated, thirty-one were cured, and eighteen underwent great amendment; there being only four who obtained no advantage from the use of the medicine. Of the infusion sweetened with sugar, two or three cupfuls were prescribed daily. This is made by placing a good pinch of the leaves into 250 grammes of boiling water. The decoction for baths, lotions, or as injections in fistulous openings, is made by boiling a small handful of leaves for ten or fifteen minutes in a kilogramme of water. The extract is made by the method of displacement. The syrup is made by mingling forty centigrammes of the extract with thirty grammes of simple syrup. To young children, two or three teaspoonfuls of this syrup may be given in the course of the twenty-four hours. The ordinary dose for adults is from thirty-two to forty grammes, and more than sixty-four grammes have been given. (Dr. Negrier, p. 22.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Coma.—Be careful, in the first instance, that the coma is not from pressure, or from inflammation of the brain, or dependent on opium or alcohol in the system. Besides these, the coma may be traumatic, epileptic, or renal epileptic, or hysterical, or rheumatic, or gouty. Always bear in mind, in cases of coma, to examine also the state of the renal and hepatic secretions: if these fail, coma is the result. As to the traumatic form, or that from concussion, the system of non-interference is the best. In the epileptic form also, the expectant plan, with moderate purging, answers better than any other. In the coma of gout, rheumatic or scarlet fever, the treatment must be of the eliminating kind, as blistering and purging, holding up at the same time the powers of the digestive organs. If there is reason to believe the blood is poisoned by urea, the hot-air bath is of service, giving also drastic purgatives, as elaterium, &c. At the same time, always remember, that, as delirium is the slighter degree, and coma the more aggravated condition of the same state, the treatment of the two conditions must be similar. (Dr. R. B. Todd, p. 86.)

Catalepsy.—The most effective means are hot pediluvia, and the cold douche applied to the head at the same time. If sensibility remains, ammonia and other stimulants may be applied to the nostrils. Use also rubefacient frictions, and administer stimulating enemata. During the interval, the cause must be sought for, and, if possible, removed. If a hemorrhoidal hemorrhage has been suppressed, leeches must be applied round the anus; or if there are worms in the intestines, anthelmintics must be given. (Dr. E. Milner, p. 99.)

EPILEPSY.—Give one ounce of the recent expressed juice of the cotyledon umbilicus twice a day for some time: if this cannot be obtained, the concentrated fluid extract, prepared by Mr. Hooper may be given; at the same time, use no fermented liquors, wear flannel next the skin, employ tepid sponging during the winter months, and cold sponging or the shower-bath in summer. (Mr. T. Salter, p. 91.)

HYSTERIA.—*Cerebral.*—Employ as a prophylactic and curative agent in this affection, the persevering and systematic application of electro-galvanism to the abdominal and pelvic regions, in combination with other means. Amongst the adjuvants that should be administered concurrently with its use, Dr. Laycock mentions tar in small doses, the introduction of which we owe to Professor Simpson, and which is in itself an excellent remedy for habitual constipation, without the aid of electro-galvanism. (Dr. Laycock, p. 88.)

DELIRIUM.—*Delirium e potu.*—The object, here is to promote the elimination of the poison without depressing the vital powers unduly. This is best done by emetics, purgatives, and sudorifics. Give the first if you have reason to believe alcohol or undigested food remains in the stomach. If the patient is violent, you may use mechanical restraint, applying, at the same time, cold to the head. Be very cautious in using blood-letting, on account of the great tendency to depression. If the stomach is highly irritable or inflamed, do not give antimony, but apply mustard cataplasms to the epigastrium. Give ice, or iced water, cold drinks, and prussic acid. If these fail, then leech the epigastrium, give morphia and creosote, effervescing draughts with potash, soda, or ammonia, keeping up at the same time a free action of the bowels, by small and often-repeated doses of sulphate of magnesia dissolved in water. In cases where the true delirium tremens is complicated with the delirium e potu, we must combine, as far as possible, the two forms of treatment. (p. 73.)

Of Bronchitis and Pneumonia.—Depression of the vital powers favours delirium. If delirium come on after antiphlogistic treatment, immediately alter the plan, and adopt a stimulating one. There is great tendency to coma, especially after the antiphlogistic treatment. If coma come on, do not relax in giving support, and you may have to give stimulants. Apply also free counter-irritation to the back of the neck; and it may be necessary to shave the head, and apply a blister to the scalp. (p. 74.)

Of Epilepsy.—There are no direct means of cutting short the paroxysm of epileptic delirium. The more innocent means, as introducing salt into the mouth, and applying cold water, may be tried. The main treatment consists in a cautious guidance through the prolonged fits. There is no necessity for local or general bleeding. If the pulse is strong, no stimulants are required; if weak and irregular, or quick and running, then they are useful. The head may be shaved; and if hot, cold applied; and small blisters applied in succession to the scalp. All means must be taken to prevent the patient injuring himself, and the gentler these are the better. If there is extreme wakefulness, give hyosciamus or hop rather than opium; and the cold douche or shower-bath is often very effectual in inducing sleep. The other drugs which may be administered are of the tonic kind, the most useful being the metallic tonics. (p. 83.)

Of Erysipelas.—The treatment of this form of delirium involves the treatment of the disease itself. Erysipelas is dependent upon the introduction of a morbid poison attacking the skin, and proceeding to the gastro-pulmonary mucous membrane, or *vice versa*, inducing the secondary formation of abscesses, or the tertiary chronic inflammation and induration of various glands, with the deposition of scrofulous matter. Can we eradicate this poison by a bold stroke at the outset? If a large dose has been imbibed, the disease must pass through a certain course; and as there are no means of extracting it, we must guide the patient through the various stages of the fever, and counteract its depressing and destructive effects. First evacuate the bowels, with as little depression as possible; then give nutritious food, with stimulants, as brandy or wine, giving them frequently and at short intervals, and adding ammonia, bark, &c., if deemed advisable. If the stomach is irritable, or the patient opposes, or is disgusted with the medicines, give up the drugs, and trust to food and brandy only. When delirium sets in, the patient wants more support and more stimulus; and the more freely they are given, the sooner the delirium will be subdued. If there is a tendency to coma, shave the head, and apply blisters freely; and if the delirium is decidedly of the active kind, henbane, camphor, and hop are safer than opium, and may be cautiously given. (p. 76.)

Of Fever.—1st. Of the low muttering kind, the patient apparently unconscious, but capable of being roused by loud speaking. 2nd. Delirium of the active kind, patient being wakeful, talkative, and needing watching, or perhaps restraint. This generally comes on very suddenly, but the first generally very gradually. In the treatment of typhus fever itself, always give nutritious food and stimulants early. As there is no inflammation present in the brain when delirium comes on in typhus fever, do not bleed even locally. True, there is congestion; but it is only part of the general tendency, and therefore stimulate the capillary circulation, so as to promote the flow of blood, which tends to stagnate in the fine blood-vessels. Apply, therefore, several small blisters to several parts of the shaven scalp in succession: if there is not time to wait, apply a large one, cutting it into slips, so as to apply it to all parts of the scalp. If there are clear reasons for taking away blood, do it rapidly by expert cupping over the temples. Be very cautious in giving opium; but if the state of congestion is not obvious, and the powers not very low, one or two doses well-timed often do great good. In the delirium of this disease, stimulants are of the greatest benefit; and give them in larger and more frequent doses. The pulse serves as a useful guide: if it does not quicken, but improves in quality, under their use, especially if it diminishes in frequency, let them be continued. (p. 79.)

Of Rheumatism and Gout.—This delirium occurs more frequently after bleeding than otherwise. But if the delirium does come on, it must not be allowed to continue, although the cardiac disease is present also. Opium is equally applicable to both, and it must be given to procure sleep, as in traumatic delirium or delirium tremens. Apply a blister to the region of the heart, and promote a free discharge from the blistered surface; avoid any bleeding or depressing treatment until all the delirious signs have passed away. Supply the patient constantly with food and with stimulants. Do not fear lest this plan should increase the cardiac affection: it appears to be the most effectual plan to cause the termination of it to be in resolution. If the antiphlogistic plan is persisted in, the delirium is increased, the powers of the patient exhausted, and effusion into the pericardial sac takes place. The delirium of acute gout needs the same treatment; but as it has a greater tendency to pass into coma, we must not be so ready to administer opium, but wait to observe whether there is any marked tendency to coma. (p. 75.)

DELIRIUM TREMENS.—1*st. Milder form.*—The great object of treatment is to restore the balance of nutrition, and procure sleep; hence regulate the diet, and give half a grain of morphia every night. As there is no inflammation to subdue, there is no need of the antiphlogistic treatment. 2. *More intense form.*—It is important to determine the cause of the disturbed nutrition, and administer the food in small quantities at a time, but frequently repeated. If the stomach will not bear solid food, or animal broths, milk may be given, combined with lime-water, or some alcoholic stimulant. To allay the irritability of the stomach, open the bowels by purgatives or enemata. Give ammonia in effervescence, or the common effervescing draught of soda or potash, or prussic acid or creasote, combined with morphia, or small quantities of iced water or ice; or apply counter-irritation over the region of the stomach. As to the means of restraint, let him be carefully watched, and persuasion used, rather than the employment of the straight-waistcoat; or call in the aid of some friend who is known to possess a powerful influence over him. To procure sleep, use crude opium or laudanum: give it, especially where there is much depression, boldly in two or three grain doses, with one or two grains of quinine. If the pupil becomes contracted, and yet he does not sleep, you may try chloroform. As a quieting means, the application of cold to the head, either by a bladder of ice, or the cold douche, may be tried, but they must not be applied too long. If you use chloroform, keep the patient horizontal; if the pulse fails or alters, desist, and always examine the heart previously. If the patient will not take food, it is very unfavourable: in this case, give enemata of some highly nutritious broth, such as strong beef-tea. If this fails, give food by the stomach-pump and œsophagus tube. In this disease do not bleed, unless there is a constant fixed pain in the head, which does not yield to the treatment, then you may bleed locally, but chiefly apply blisters or other counter-irritants. There is no occasion to give mercurials. If there is a tendency to coma, give up opium, and examine the urine if albumen be found it. The coma may be depending on the slow elimination of urea. Apply blisters in this case to the back of the neck or head, and keep them discharging; at the same time promote the action of the skin and bowels as much as the strength permits. (Dr. R. B. Todd, p. 66.)

NEURALGIA.—The 'Journal de Pharmacie' recommends sixty drops of chloroform to be rubbed up with one ounce of hog's lard, and two or three frictions a day applied over the painful spot. It should be kept from the light and air. (p. 93.)

POISONING *by Strychnine* —A case in which all the symptoms of poisoning by strychnine were present, was very successfully treated by the inhalation of chloroform. (Dr. Munson, p. 417.)

SCIATICA.—The French journals have recorded cases of the cure of sciatica by cauterising the ear on the anterior part of the right helix, and it would appear, from their statements, that the practice was of considerable antiquity. (p. 93.)

TETANUS.—We are justified, with the view of removing the exciting cause, in amputating the entire part, or dividing the nerves leading to it. In tetanus the energies of the brain are *minus*, and those of the spinal marrow *plus;* therefore we must increase the first by stimulants, wine, brandy, and Indian hemp, and reduce the latter by the careful use of tobacco enemata and fomentations (fifteen grs. of tobacco to eight ounces of boiling water) every half hour, so as to keep up a state of nausea. The spine should also be well rubbed with a liniment of croton oil and turpentine. It is right to say that idiosyncracy in some instances renders the use of tobacco extremely dangerous. The symptoms in this case, are, the countenance assuming a deadly hue and ghastly appearance, and the pulse becoming quivering and intermittent. When these occur the administration should be stopped, and stimulants immediately resorted to. These means, with paying attention to the state of the bowels by croton oil or other strong purgative, and bringing the patient under the influence of tobacco, as rapidly as possible, are the principal means upon which we have to rely. (Mr. H. R. de Ricci, p. 96.)

Traumatic.—Use frictions with the tincture of belladonna, composed of five parts of extract to eleven of alcohol, and apply them all over the body, more particularly over the rigid parts. (M. Bresse, p. 99.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

ANGINA PECTORIS.—In addition to the remedies of books, special attention must be given to the inhalation of oxygen, and to the use of electricity. (Dr. Kneeland, p. 142.)

HEART.—*Disease of, and Dropsy following.*—In warding off the symptoms or feelings, too often concomitant with disease of the heart,—next to quietness of mind and body, equable temperature, and moderate or rather low living,—there is nothing equals the use of small doses of colchicum wine, weak saline purges, and inserting a seton over the region of the heart. In a sudden paroxysm of violent dyspnœa with feelings of immediate suffocation, no means are equal to abstracting from two to four ounces of blood from the arm. In the treatment of cardiac dropsy, the following diuretic, which will pump the patient out, so to speak, sometimes in a few hours, is recommended, and it is often of equal value in repeated attacks of of the anasarca. ℞. Infusi digitalis, ℥iv; potassæ acetat., ʒij; sp. æth. nitrosi, ʒij; aquæ cassiæ, ℥iss. Capiat cochleare magnum quartâ quâque horâ. If these remedies (diuretics) fail, we must fall back upon purgatives, elaterium chiefly:—℞. Elaterii, gr. j; extracti coloc. comp. ℈iis; extracti hyosciami, gr. xij. M. Divide in pilulas xij. Capiat unam nocte maneque. The combination of tartrate of potass, or the tartrate of potass and soda with the infusion of senna, is highly efficient. Puncturing the extremities and thereby draining off the fluid, should not be had recourse to until all internal remedies have failed; though whether as a palliative or as a hope of cure, it deserves more notice than it commonly gets. Although fomentations are more generally advised, yet the evaporating lotion has been found very successful in keeping down any disposition in the part to erysipelas. (Dr. A. Kilgour, p. 109.)

Fatty degeneration of.—As we cannot restore muscular fibres that have been destroyed, we can only hope to arrest or suspend for a time the progress of the disease by improving the quality of the blood, and thus supporting the vigour of those portions of the heart still uninjured. We must pay attention, in the first place, to the digestive organs, give bitter tonics and alkalies, and subsequently iron in different forms, and carefully regulate the habits of life. For the pain and the distressing attacks of angina pectoris, the greatest relief followed the repeated application of three or four leeches, succeeded by a blister, and after these the internal use of iron. Be careful in recommending exercise. Narcotics are given with great risk, but it is almost needless to indicate the usefulness of antispasmodic remedies during the paroxysmal attacks. (Dr. R. Quain, p. 102.)

HEMORRHAGE.—*From Leech-bites.*—This may be arrested by the application of caustic.. To apply this properly, crush a small piece of lunar caustic to powder, heat the point of a silver probe in a candle, and dip it into the powder; it thus becomes crusted over with the nitrate, and forms a fine button of caustic, which will easily reach the bottom of any such wound. (Dr. D. Maclagan, p. 441.)

From Punctured Wounds.—Apply caustic in the manner recommended by Dr. Maclagan. See "*Hemorrhage from Leech-bites.*"

From Wounds of the Palmar Arch.—Flex the arm on the forearm at an acute angle, and retain it there either until the proper means can be used for securing the vessels, or until the wound has entirely healed. (Mr. E. Durwell, p. 263.)

PURPURA HEMORRHAGICA.—Although purpura is certainly a disease connected with a dissolved and thin condition of the blood, yet it is also more than probable, that weakness of the vessels themselves, from defective nutrition of their walls, has a still larger share in the result; the symmetrical distribution of the ecchymoses, proving the correctness of this supposition. On this ground, the very valuable properties of turpentine as a styptic, both applied locally and administered internally, is strongly recommended, great benefit having been produced by it, when all other measures had failed. Twenty minims of the oil may be given in emulsion every six hours. A similar testimony of the styptic properties of turpentine is quoted from the illustrious Hunter, and also from Mr. Vincent, late surgeon to St. Bartholomew's Hospital. (Dr. W. Budd, p. 137.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Angina Tonsillaris.—Dr. Plange states that the employment of the zincum aceticum in this disease, and especially in thirty cases occurring during an epidemic of scarlatina, where it was exhibited in very different degrees of the affection, has been followed by almost immediate relief. He prescribes from ℈j. to ʒj. in from ℥vi. to ℥viij. of water, giving a tablespoonful in some mucilage every two hours, in severe cases, and frequently gargling the throat also with the same.—*Med. Times, Aug.* 10, 1850, *p.* 150.

Asthma.—Give very minute doses of tartar emetic (from three to six pills, each containing 1-25th of a grain of tartar emetic every twenty-four hours). (M. Bernardeau, p. 173.)

Diphtherite.—Apply the solution of caustic freely over the whole surface; the inflammatory crust is destroyed, and a more healthy surface remains. The repetition of the application two or three times a day is required. In scarlatina anginosa the same may be employed with good effect. The solution may be made of from 40 to 60 grains of crystallized nitrate of silver to ℥j. of distilled water. (Dr. Green.) —*Monthly Journal of Med. Science, July,* 1850, *p.* 15.

Hemoptysis.—*Tubercular.*—An excellent remedy is gallic acid, in doses of two or three grains. (M. Homburger, p. 427.)

Laryngo Tracheitis.—[Dr. James Gillespie records a case of this disease, in which the operation of tracheotomy was resorted to as a last resource, and with perfect success.] (p. 179.)

Larynx.—The structural changes of the larynx are,—inflammation, which may terminate in ulceration, hypertrophy, induration, or deposition of tuberculous matter in the follicles themselves. This, from its frequency among the clergy in America is known as the clergyman's sore throat. Cauterize freely the whole diseased surfaces with a strong solution of crystallized nitrate of silver, consisting of from 40 to 60 grains to ℥j. of distilled water. The tongue is depressed, and the epiglottis brought into view by means of a jointed spatula. A sponge attached to a curved rod of whalebone is carried steadily, without touching the pharynx, behind the erect epiglottis, drawn slightly forwards and pressed steadily downwards, when it readily slips into the glottis, which is known both by the feeling of gasping, and the spasm immediately induced in the patient. By causing the patient to respire fully the operation is facilitated. It may be necessary to pass it fairly into the aperture, and down to the chordæ vocales, which is in general accomplished with little difficulty. Dr. Scott, who has repeatedly followed this practice, has never seen it produce any disagreeable symptoms. (Dr. Green, p. 174.)

[We think, however, that this practice of passing the sponge into the larynx must be attended with danger.—Ed. Retrospect.]

Phthisis.—Tartar emetic has been used with excellent effect in this disease. Give from three to six pills in the 24 hours, each containing 1-25th of a grain. By their use the cough, dyspnæa, and inordinate action of the heart become calmed, and, in fact, all the good effects of morphia, without its inconveniences, seem to be produced. (M. Bernardeau, p. 173.)

Stethometer.—[An instrument for measuring the capacity of the movements of the chest has lately been introduced by Dr. R. Quain, which he calls the "Stethometer." A full description of this instrument is given at page 187.]

Trachea.—To apply nitrate of silver to the trachea, use an instrument having a small grindstone, five or six inches in diameter, which, being caused to be rapidly revolved by means of a pulley and strap, and the salt being placed upon it, a fine dust is thrown from it, which, by the mouth being opened and the breath drawn in, is placed in direct contact with the diseased surface of the trachea or larynx. (Dr. J. Chew Thomas, p. 179.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

Ascites.—At the suggestion of a French journal, it has been found that, after all other means have failed, the application of diuretics locally remove the fluid of ascites. A mixture of equal parts of tincture of digitalis, tincture of squill, and tincture of soap, was rubbed freely and diligently into the skin of the belly, morning and evening. A copious flow of urine soon took place, and in 14 days the ascites had entirely disappeared. In another case, where the liniment could not be used, Dr. Duncan applied linen cloths soaked in the pharmaceutical infusion of digitalis, with perfect success. The infusion may be applied by means of the spongio-piline.—(Dr. R. Christison, p. 139.)

Dr. Todd lays great stress on the success which has followed the treatment of numerous cases of ascites from pressure on the abdomen, by means of strapping and bandages. (p. 192.)

Epistaxis.—A tube of caoutchouc with a dilatable extremity, or balloon, has been invented by M. Gariel, which, by being introduced into the nostril and expanded by insufflation, exerts such pressure as to arrest the hemorrhage in cases of epistaxis. By being carried into the back part of the mouth along the floor of the nares, and then expanded, the posterior nares may be also plugged. M. Diday, of Lyons, has also made an ingenious use of a similar dilatable tube, by introducing it into the uterus, and expanding it, in repressing uterine hemorrhage. (p. 269.)

HARE-LIP.—The French surgeons operate on children a few days after birth; and it is stated that at that period they do not struggle, union is obtained very rapidly, the little creatures cry but little, take the breast well, and do not seem to suffer much. (p. 267.)

HEMORRHOIDS.—Administer two ounces of fresh linseed oil, morning and evening. The amendment is generally so rapid that the remedy is seldom continued longer than a week. The only precaution the while being the abstinence from alcoholic drinks and stimulating diet. (M. Van Ryn, p. 273.)

HERNIA.—The following conclusions of a practical nature from attentively investigating the varieties of arterial distribution in relation to hernia have been arrived at by Dr. Redfern. In inguinal hernia divide the stricture upwards. The risk of hemorrhage is greater in operating for femoral than for inguinal hernia. Divide the stricture of a femoral hernia inwards, or upwards and inwards. Divide as few fibres as possible; avoid a sawing motion; and press the edge of the knife rather less on the anterior surface than directly on the edge of the fibres constituting the stricture. If the structure of the neck of the sac is divided upwards, the spermatic artery may be wounded, and the testicle lost. (p. 264.)

LIVER, *Torpor of.*—The greatest possible difficulty meets us in the treatment of this state, occurring in those who return from tropical to temperate climates. What little bile is secreted being vitiated, our indications of treatment should be directed to the production of a more abundant and healthy action, and to the attenuation and elimination of the fluid secreted. Use the fluid extract of taraxacum, with the carbonates of soda and potash, or with the bitartrate of potash, which affects the excretion from the kidneys, increasing it in quantity, and modifying it in quality, while they exert a solvent power on viscid bile, on fatty and other animal substances, and correct the acidity of chyme. If the diuretic effect is wished to be increased, add the acetate of potash, or the benzoate of ammonia. If alkalies prove cold or ungrateful, substitute the dilute nitric and hydrochloric acids, especially where tropical dysentery or diarrhœa has previously existed, or where an irritable state of the mucous membrane is present. Aloes seems to be more valuable with the above combinations than any other purgative. If anæmia is present, restore the nutritive functions, if possible, and improve the state of the blood, by the alteratives already mentioned, bitter tonics, chalybeates, with mild aperients, and great attention to the diet and habits of life. If there is reason to believe that organic changes have taken place in the liver, or that it is accompanied with induration and enlargement of the spleen, use the nitro-muriatic acid bath, as it acts powerfully in promoting the functions of the liver, kidneys, bowels, and skin. The nitro-muriatic acid, diluted, is made by mixing three parts of hydrochloric acid, two of nitric acid, and five of water: and of this dilute acid, three ounces to each gallon of water form a bath; two gallons will form a foot-bath; while the outside of the legs and thighs, the right side over the liver, and the inside of both arms, are sponged alternately. Let a dose of Epsom salts be taken in some bitter infusion every other morning, while the patient is in the bath. If there is dryness or harshness of the skin, use a vapour bath twice a week. If the bath is used for the whole body, excite gentle perspiration previously, by covering the patient with hot blankets. If it excites irritation of the skin, let the quantity of the acid be diminished. If irritation of the gums, with general malaise is produced, relinquish it for a time. This bath may used for two months, or longer in the severer cases. The neglect of this invaluable remedy is very much to be regretted. It seems to be equally valuable in the secondary and tertiary forms of syphilis; and another valuable quality is, that the use of opium is not contra-indicated during its exhibition. We obtain from the use of these mineral acids all the remedial effects in chronic, which we do from mercury in acute diseases, without its injurious effects.—(Mr. James Ranald Martin.)—*Lancet, June* 22, 1850, *p.* 749.

PROLAPSUS ANI.—Five different means of treating this affection are mentioned by M. Dieffenbach in his operative surgery. 1st. By diminishing the anal opening by excision of folds around it; 2nd. By excision of wedge-shaped pieces from the anus; 3rd. By excision of parts of the anal ring, and of the callous prolapsus; 4th. By extirpation of the spongy prolapsus; and 5thly. By cauterization. The respective merits of these different methods are fully discussed at page 271.

RECTUM.—*Fungous Tumour of.*—This affection in children, attended by bloody discharges, which M. Leclayse believes is often mistaken for hemorrhoids, M. Martin has repeatedly and successfully treated by the application of the nitrate of silver. (p. 271.)

Stricture of.—An operation for this disease was performed at St. Thomas's Hospital, as follows:—The patient being placed upon her face, the stricture soon reached, the mere point of the finger getting into it, a grooved director was passed up to it, together with a bistoury. The director was next let fall out, and the knife carried through all the parts up to the coccyx. The intestine above the point of the stricture seemed quite free. The rectum was laid open, and two vessels, probably branches of the superior hemorrhoidal, spouted out pretty freely, and were tied; the fibres of the upper and lower sphincter were also visible. The thick mucous membrane with the stricture, and the various veins and nerves were all divided. After the hemorrhage had ceased, a large plug of lint was put into the wound, and secured by a T bandage. The operation was quite successful. (Mr. J. Simon, p. 269.)

TONGUE.—*Inflammatory and other affections of.*—Dr. Fleming says, that sudden and alarming swelling sometimes takes place in the tongue, which seems to be merely an unaccountable and active hyperæmia, readily yielding before inflammation has had time to be lighted up, by incision, by leeching copiously, both locally and under the chin. Another affection is "an inflammation, circumscribed or diffused, originating in the loose cellular tissue between the genio-hyo-glossi muscles," the treatment for which is antiphlogistic, but if it does not readily yield, Dr. Fleming recommends a free incision to be made under the chin in the median line, through the integuments and fascia, and through the raphé of those muscles, delaying the advancement of the suppurating process. Dr. Fleming says, the best treatment for the abraded surface of the tongue, sometimes met with, and for a peculiar kind of ulcer, which is accompanied by a small tumour about the size of a pea, and which he states may occur without the slightest suspicion of a syphilitic taint, is the iodide of iron, with hemlock, and the local application of the nitrate of copper. Dr. Fleming thinks it a tuberculous disease, and says it is by no means uncommonly met with. The nitrate of copper, he says, is almost invaluable also as an application to the small excoriated ulcers, of a semi-phagedenic character, occurring in the genitals of both male and female. It is very deliquescent, and can be applied only in its liquid state. The surface of the ulcer should be well dried previously, and afterwards covered with oil.—*Dublin Quart. Journal, August*, 1850 *p.* 87.

TEETH.—*Vegetable and Animal Parasites of.*—From microscopic observations made by Dr. Bowditch, he has, in a large majority of instances, found these parasites between the teeth, or at the juncture of the gums, in persons from different classes of the community, but without any disease of the mouth. He attributes their presence to a want of cleanliness; and recommends thoroughly brushing the teeth after each meal. M. Foy, a French dentist, recommends tinct. kino, tinct. catechu, āā; a teaspoonful to be added to cold or tepid water, and used every morning.—*Brit. and For. Med. Chir. Rev., Oct.* 1850, *p.* 554.

TONSIL.—*Enlarged.*—The excision of enlarged tonsils is recommended, as performed by M. Lisfranc, of Paris, by grasping the tonsil with the forceps, pulling it inwards from the side of the fauces, or merely held steady in its natural position, and then passing a straight narrow blunt-pointed bistoury, the blade being sheathed except rather more than an inch from the point, through its base. It may be done with perfect safety, with immediate relief, and with little hemorrhage succeeding. (Prof. Syme, p. 268.)

Mr. Harvey recommends the bichloride of mercury, in small and divided doses, with tincture of rhubarb and of bark, to be taken at bedtime; also the tincture of colchicum, to be taken internally, and and applied externally with lin. saponis. When the scrofulous character predominates, he uses also the cod-liver oil. Of all the remedies advised to be used in the chronic state, Mr. Harvey thinks colchicum is the best. He was led to this conclusion from investigating the history of this affection, and the contents of the tonsils, resembling very much those concretions found in the joints of gouty and rheumatic patients.—*London Jour. of Medicine, Sept.* 1850, *p.* 850.

Worms.—*Tape-worm.*—Order the patients to live sparingly, and take a dose of castor oil the day previously, and then give a dose of kousso, giving also a dose of castor oil soon after it to carry it down to the worm, and expel the dead or enfeebled worm from the bowels. This medicine is prepared in separate doses by Mr. Hooper, Pall Mall, for 16s. to 20s. a dose.

[Dr. Inglis, of Halifax, has also recorded a perfectly successful case of expulsion of tape-worm by the administration of the kousso. (p. 199.)]

AFFECTIONS OF THE URINARY ORGANS.

Bladder, *Chronic Catarrh of.*—M. Ricord uses caustic injections, ʒij of nitrate of silver to ℥iv of distilled water. He repeats the injections every three, four, or six days. (Mr. W. Acton, p. 331.)

Paralysis of.—M. Lecluyse has published some cases in which all ordinary means appearing useless in removing this affection, he gave strychnia internally, but without any benefit to the paralysis. He then dissolved six grains in alcohol, mixed it with a pint of water, and ordered four injections of ℥ij each to be introduced through the catheter, the bladder being previously emptied. It was followed by complete success. But in another case under M. Robert, this gentleman ordered three grains in six ounces of water. This not being soluble, the pupil in charge substituted the sulphate of strychnia for the alkali itself. The injection was given, but symptoms of poisoning soon came on, and the patient ultimately died from the effects of the strychnia upon the system. No appreciable change was remarked in the muscular coat of the bladder, it remained perfectly paralyzed.—(p. 227.)

Great benefit will be found to result in this disease from the application of electricity as follows:—A silver catheter is introduced into the bladder, and a female catheter introduced into the rectum, and made to rest upon the recto-vesical parietes; each of these catheters is connected with one of the poles of an electric machine, which is put at first in gentle action, and then continued briskly for two or three minutes. (M. Michon, p. 228.)

Hematuria.—Give gallic acid in two or three grain doses. (Mr. Homburger, p. 427.)

Phymosis.—M. Vidal operates for phymosis, by tracing with ink on the prepuce, a line indicating the point where the section is about to be made, then grasping the prepuce with a dressing forceps, in front of this line, he draws the prepuce a little forward, and, having passed three needles with a waxed thread through it, at the line traced by the ink, he divides the mucous membrane and skin at one cut. (p. 296.)

Retention of Urine.—Mr. Tatum, of St. George's Hospital, reports a case in which retention of urine had taken place from stricture. All means of passing the catheter both before and after the warm bath, and after the most patient and cautious manipulation in trying to accomplish this object had failed, and it was only after a large dose of laudanum had been twice given, and the warm bath repeated with a soothing enema, that the catheter could be introduced. The symptoms again coming on after some time, the same means were again repeated, but it was not before a purgative draught had been given, eight leeches applied to the perineum, and the opium and warm bath again resorted to, that the stricture yielded so as to allow the instrument to be passed, and the bladder to be emptied. (p. 292.)

Mr. Holt, of the Westminster Hospital, reports a similar case coming under his case, where the same means and the same perseverance were rewarded with success. (p. 287.)

When the bladder cannot be evacuated by the catheter, after leeching, aperients, warm baths, and opium, are we to puncture the urethra behind the strictured portion (Sir A. Cooper),—or make a free opening in the perineum over the strictured part (Liston),—or perforate the bladder through the rectum? Mr. Gay, of the Royal Free Hospital, advocates the latter, and states that he has never met with one unfavourable result from the practice. (p. 289.)

A new method of relieving retention, without using the catheter, is as follows:—Clear out the intestines by an enema, then administer as an enema a quart of cold water. Absolute rest must be enforced, and apply bladders of ice, cold water, or pounded ice to the anus, perineum, thighs, and hypogastrium. If this does not succeed, let a stream of cold water be poured upon the region of the bladder during twenty to twenty-five minutes. After a time, repeat the cold applications as above. This mode is chiefly valuable where the retention proceeds from spasm, or acute inflammation. (M. J. J. Cazenave, p. 291.)

STRICTURE OF THE URETHRA.—In three cases at Guy's Hospital, which, in spite of all ordinary dilating means, soon resumed their former strictured condition when these had been removed for a short time, the operation of perineal section was performed, as was recommended by Mr. Syme. In one, an extremely irritable and nervous subject, the issue was unfortunately fatal; but in the other two, complete success resulted, and the patients passed their water without any difficulty. (Mr. Cock, p. 274.)

In three cases of stricture of the urethra operated upon by Dr. James Dunsmore, in the Royal Infirmary, Edinburgh, by external incision, as recommended by Mr. Syme, no conclusion could be drawn from the first case either for or against the operation; but in the other two the result was perfectly successful: and so satisfied is he of its advantages, that he intends adopting it for the future in all cases of bad stricture coming under his care. (p. 283.)

In ordinary cases, M. Ricord employs simple dilatation by means of conical gum-elastic bougies with a little olive-shaped point. When retraction takes place, the case is treated by incision with instruments passed down the urethra. (Mr. W. Acton, p. 332.)

AFFECTIONS OF THE SKIN, &c.

ABSCESSES.—In opening abscesses, if the attenuated skin is of a circular form, so that when divided by the knife in any direction it must leave broad thin flaps, apply caustic round the edge where the healthy and thin skin meet; and the whole of the latter being, as it were, included in a ring of eschar, will perish. This plan answers well in large buboes, especially in scrofulous subjects. If the abscess has opened spontaneously, and there is much unhealthy skin, and also unhealthy granular and cellular tissue below it, use the caustic at once, as all this has to be got rid of before a cure can be accomplished. (p. 435.)

CONTRACTILE CICATRIX.—To close preternatural openings which have lost all tendency to contract naturally, if the openings are small, with callous edges, apply the nitrate of silver. If they are large, pare the edges, and bring them together by suture; and if a small opening should still remain, then the application of caustic may complete the cure. (Dr. D. Maclagan, p. 442.)

ECZEMA.—A remedy which has been found more extensively applicable and more uniformly serviceable than any other, is a solution of two drachms of the sesqui-carbonate of soda in a pint and a half of water; but it is necessary to place lint saturated with the solution over the affected part, and to cover the whole with oil-silk, in order to prevent evaporation. In these cases there is an increased exudation from the skin, both of sebaceous and purulent matters; and alkalies, we know, have the property of dissolving these, and of acting as a calmative and emollient to the irritated parts. (Prof. Bennett, p. 305.)

ERYSIPELAS.—Mr. Lawrence recommends the antiphlogistic treatment; Dr. Fordyce the stimulating plan; Desault gives a grain of tartar emetic in a considerable quantity of fluid in the bilious erysipelas. There is no form of the disease which should not be attacked from the first with tartar emetic, and under all and every circumstance we shall find that the disease yields to this remedy. It should be administered in small doses (one grain dissolved in any bland fluid being taken in the twenty-four hours), which may be repeated three or four times, so as to keep up its effect on the disease, on which it appears to act as a specific. As a general rule when the erysipelatous surface is getting a yellow tinge, the skin shrivelling a little, and the pulse becoming more frequent (as it generally falls under the use of the tartar emetic), the proper time has arrived to commence with tonics and omit the tartar emetic. Of these the sulphate of quina has been found the most successful. In some cases where there are low typhoid symptoms, with prostration of strength, tonics and stimulants may have to be given with the tartar emetic, even from the commencement. Pil. rhei co., gr. vj, with pil. hydrarg., gr. ij may be given as an aperient, to be repeated in six or eight hours if necessary. All local applications are unnecessary except in idiopathic erysipelas, in which flour or powdered starch may be used, and in the traumatic form, a large linseed-meal cataplasm. There is rarely any occasion for leeching, or the making of incisions, if the antimonial treatment be early adopted. Erysipelas being a constitutional disease, local treatment is of little use; "inflammation being the method taken to throw off the morbid state of the blood, anything that tends to check that eruption will be the means of preventing the efforts of nature, and probably cause a metastasis to some other part of the body." (Dr. A. J. Walsh, p. 24.)

Apply collodion over the inflamed and neighbouring parts. It acts by compressing the capillaries of the skin, and thereby contributing materially in relieving those vessels. The results of this treatment have been highly satisfactory. (Mr. J. Luke, p. 325.)

Favus.—By those not well accustomed to the diagnosis of skin diseases, favus has often been confounded with other eruptions of the scalp, more especially eczema and impetigo, or the combination of these diseases known as the eczema impetiginodes; but in none of these do the yellow crusts or scales present, when examined microscopically, traces of vegetation. This furnishes the real diagnostic and pathognomonic character of the disease. There can be no doubt the disease is inoculable, and capable of being communicated by contagion. The pathology of favus is best understood by considering it essentially to be a form of anormal nutrition, with exudation of a matter analogous to, if not identical with, that of tubercle, which constitutes a soil for the germination of cryptogamic plants, the presence of which, as stated, is pathognomonic of the disease. As favus is in many cases a constitutional disease, and dependent upon the causes inducing scrofulous diseases in general, the treatment must be constitutional, and directed to remove the tendency to tubercular exudation. The internal and external exhibition of cod-liver oil with appropriate diet and exercise, has been attended by most marked advantage. As to the local treatment, Professor Bennett directs the affected scalp to be poulticed for several days, until the favus crusts are thoroughly softened and fall off, then the head to be carefully shaved, and lastly, cod-liver oil to be applied with a soft brush, night and morning, and the head covered with an oil-skin cap to prevent evaporation, and further exclude the atmospheric air. Every now and then the accumulated and inspissated oil should be removed by gently washing it with soap and water. Whenever favus is recent, and of limited extent, it may be at once destroyed by cauterization with nitrate of silver. (Dr. Bennett, p. 279.)

Lupus.—Dr. Thomson relied on three medicines—iron, iodine, and arsenic, with biniodide of mercury, and the occasional use of cod-liver oil and quinine. If the general health was at all lowered, and if there were any anæmic symptoms, cod-liver oil and iodide of iron were given for ten or fourteen days. After this, the alterative treatment was commenced, by giving the biniodide of arsenic in 1-12th of a grain. If gastrodynia came on, the medicine was left off, and opium and hydrocyanic acid administered. Conium was also found useful, and frequently mixed with the biniodide. If the latter could not be borne, then the liq. potassæ arsenitis was given in small doses, from four to eight minims, and iodine was rubbed in over the healthy skin to promote absorption. Dr. T. occasionally applied the strong nitric acid to the edges of the ulcer, or a strong solution of the nitrate of silver (ʒ ij or ʒ iij to ℥j of water) over the tubercle, and subsequently acetate of lead wash to lessen the temporary heat and swelling which followed. If unhealthy pale fungous granulations arose, an ointment of iodide of sulphur, or a very weak ointment of the biniodide of arsenic, or of either of the iodides of mercury, was employed. (Dr. Thomson, p. 320.)

NÆVUS.—[Mr. Fergusson treats this affection by strangulating it by a peculiar knot. For the method in which he operates see page 408.]

PSORIASIS. —"Psoriasis, so called, lepra vulgaris, psoriasis diffusa, psoriasis gyrata, psoriasis guttata, and psoriasis inveterata, are one disease, more or less chronic." The real nature of psoriasis is altogether unknown. There is no exudation, properly so called; there is considerable redness and an increased growth, or an hypertrophy of the epidermis. As there is generally a deranged state of the digestive organs, with sometimes constitutional disorder in this affection, we must correct these by suitable combinations. For the constitutional treatment, he advises equal parts of Fowler's solution and tincture of cantharides, in doses commencing with ten drops, gradually increased to fifteen or twenty. Most dermatologists are agreed that the best local application is the pitch ointment (ung. picis); if it causes considerable irritation it may be diluted with an equal part of lard. Some cases require only the arsenic, others the pitch ointment alone, and a third class of cases require the action of both. (Prof. Bennett, p. 306.)

And Lepra-Vulgaris.—Arsenical preparations, and especially Fowler's solution, are the best internal remedies. Tar takes the first place as an external application. The combination of these it the best treatment for psoriasis. The iodide of mercury ointment is occasionally useful when judiciously applied, and next to it the iodide of sulphur. The tar ointment is made by mixing a third or a fourth of a part of tar to two or three parts of axunge. The ointment of iodide of mercury is made by mixing one part of proto-iodide of mercury with eight of axunge; and that of the iodide of sulphur by mixing from one part in thirty-two to one part in eight of lard. Besides these, dietetical and hygienic measures must be adopted. Diet mild and not too substantial, avoiding all aliments difficult of digestion, regular exercise, frequent use of baths, and having recourse again to the treatment which had benefited them before, on the least reappearance of the disease. (M. Emery, p. 307.)

SKIN DISEASES.—*Arsenic.*—The unfavourable states of the system to the administration of arsenic are, 1st. A febrile state, especially the sthenic form of pyrexia, indicated by the usual signs; 2nd. A condition the very reverse of this febrile state is likewise unfavourable: 3rd. A syphilitic taint; 4th. The existence of organic visceral disease, as a complication of eruptions, an attack of diarrhœa, bronchitis, cynanche, coryza, or severe irritation in any mucous membrane. Certain conditions of the nervous system, especially those connected with dyspepsia, and nearly all other disorders of the general system, generally contra-indicate the use of arsenic: but if the health can be restored by other means, it may afterwards be administered with advantage. Mr. Hunt also gives some cautions as to the mode of giving it. He says, as we wish it to enter the general circulation, it should be taken on a full stomach, as it then directly enters the circulation with the chyle, being absorbed by the lacteals; if it is taken on an empty stomach it would be chiefly absorbed by the venous capillaries, and enter the portal circulation. It should be taken after a meal, also, to obviate any irritation it might produce in the stomach and bowels. Again, as arsenic is a cumulative medicine, it should never be administered in increasing doses, otherwise a sudden and sometimes an alarming development of its toxical effects will necessitate an entire suspension of the medicine. The best plan is to begin with five minims of Fowler's solution three times a day, and continue the dose steadily until the conjunctiva or tarsi become slightly affected; then reduce the dose again and again, as the cumulative action becomes apparent in the state of the tarsi. (Mr. T. Hunt, p. 313.)

Medicated Soaps.—[Sir H. Marsh has recently brought into notice various medicated soaps, which he recommends in some chronic forms of skin disease, as chronic eczema, psoriasis, &c. At page 327 of this volume are given various formulæ, in which sulphur, white and red precipitate, and corrosive sublimate, may thus be applied, and Sir H. Marsh states that, after a little perseverance in their use, the effects were spoken of by the patients as being very soothing and beneficial.]

TUMOURS.—*Erectile, in Children.*—A good method of treating these tumours, which consist essentially of a congeries of enlarged blood-vessels, is by caustic. Pass a red-hot surgical needle, which may be readily heated by a gas-flame, once or twice through the base of the tumour. The object is to cause effusion of lymph, and thus to alter the structure of the tumour, and arrest its growth. (Dr. D. Maclagan, p. 438.)

ULCERS.—*Of the Leg.*—In those cases "depending upon debility, with want of action in the system, circulation languid, wound sluggish, the surface smooth, without granulations, and of a greenish foul appearance, Mr. Hancock gives the following mixture—sp. terebinth. ʒ vj; pulv. acaciæ. ʒ vj; aq. menth. pip., ℥ viij. Cap. ℥j ter die, with a generous diet and a liberal allowance of wine, linseed meal poultices applied for a day or two, and then water dressing, with bandaging up to the knee throughout the case.—*Med. Times, July* 6, 1850, *p.* 12.

Varicose.—After all other measures have failed in the treatment of this affection, in cases at St. Thomas's Hospital, perfect success has followed the application of a caustic issue, by rubbing potassa fusa along the chief vein between the ulcer and the heart. (Dr. C. Kidd, p. 321.)

Warty Ulcers of Marjolin.—Chloride of zinc has been used in this disease, at the suggestion of Dr. Robert Smith, with remarkable success. On the first application the chloride was mixed with flour, but being too weak in this manner, it was mixed with two parts of pure sulphate of lime. The whole of the morbid growth was covered to the depth of a third of an inch with this paste. The severe pain it gave rise to was only partially under the control of opium. (Dr. Fearnside, p. 312.)

WOUNDS.—*Union of.*—M. Vidal has contrived a number of minute forceps of different shapes, whereby the lips of wounds, especially after the operation for phymosis, being gently kept in apposition by their self-applying pressure, the use of sutures is rendered unnecessary, the pressure exerted not being so great as to perforate the skin. The period for their application does not exceed twenty-four hours. (M. Vidal, p. 325.)

Poisoned.—Having removed the poison as much as possible by suction, encouraging the part to bleed, or excision of the whole tract of the wound, apply caustic to the surface freely. Venereal chancre may be regarded as a poisoned wound, and according to Ricord and others, if it is cauterized during the first three or four days, constitutional symptoms never follow, as the surface secreting the poison is destroyed, and the virus is thus prevented getting into the system. (Dr. D. Maclagan, p. 439.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

ANCHYLOSIS, *Peculiar mode of obtaining.* In cases of diseased joints, attended by suppuration, and also in chronic diseases of the joints, namely, where destruction of the articulation has taken place, and nothing can be obtained (supposing amputation be not performed) but a stiff joint, the best plan is that of freely opening the joint at once. In the first place, the consequences of opening diseased joints are not so serious as are usually represented, but, on the contrary. are so slight as scarcely to deserve notice; in the 2nd, the process by which nature cures a joint in which the cartilages are removed by by disease, is anchylosis, but this eventual result demands the absence of cartilage; and lastly, the articulation is placed in the condition of a sinus which has been slit up. (Mr. Gay, p. 241.)

DISLOCATION *of Lower Jaw.*—It is not on the condyle that we must fix attention to find the cause which renders the dislocation permanent, but on the coronoid process and the malar bone, since it is in the contact of these two bones that almost all the difficulty of reduction resides. And therefore to succeed, according to the process of M. Nélaton, it is necessary to act either by the interior of the mouth, taking a point of support behind the mastoid processes, or externally, by the operator taking a position behind the patient, and making pressure on the coronoid process, pushing it downwards and backwards, to disengage it from contact with the malar bone, at the same time the patient opens the mouth. This is effected by pressing in the direction mentioned, placing the thumb upon the coronoid processes. If this force is not sufficient, the head must be supported by an assistant; or a band may be passed around it, in which the operator can engage his index and middle fingers, while the thumb must be brought to bear on the coronoid process. (M. Nélaton, p. 260.)

Of Thumb.—An instrument in the shape of a pair of forceps, with a contrivance at the end by which the thumb can be grasped, has been devised by M. Luër, and modified by M. Charriere, and is said to be of great service in effecting the reduction. (M. Blandin, p. 262.)

FRACTURE *of the Leg.*—[Mr. Salter, late house-surgeon to King's College Hospital, has recently introduced a new swing apparatus for the treatment of fracture of the leg, and the use of which, he says, is attended by great benefit and immense comfort to the patient. This apparatus is fully described at page 256.]

Of the Ribs.—Pass large strips of adhesive plaster round the top of the chest, down over the site of the injury to about midway between the last rib and crest of the ilium; enjoin absolute quiet (not a word to be spoken). The strapping carefully managed, and the administration of tartrate of antimony sedulously attended to—especially if perspiration break out the rapidity with which these cases do well is quite wonderful. (Mr. Hilton, p. 252.)

Ununited.—This was effected by means of gradual extension being made by an instrument firmly grasping the thigh above the condyles of the femur, the foot below having a screw, by means of which the distance between the knee and foot could be gradually increased, the tendo-achillis being divided. During this time also steady continued pressure was kept up on the tibia above the fractured part, and counter-pressure on the opposite side of the limb. When the union was strong enough it was put up in common splints and well supported. (Mr. R. W. Tamplin, p. 251.)

JOINTS, *Chronic Disease of.*—In chronic disease of the joints, connected with gout and rheumatic gout, much may be done to prevent its progress at its very commencement. Place the patient on a careful system of diet, partaking very moderately of animal food, avoiding fruit, acids, &c., and taking little or no fermented or spirituous liquors. Perspiration should be induced by exercise, or by the hot-air bath, once in a week or fortnight. Acetic extract of colchicum, in alterative doses, combined with mercurial pill, and occasional purgatives, should be given from time to time. Moderate doses of potash and magnesia (always avoiding soda) may be given three or four hours after the principal meals, to neutralize any superabundant acid in the stomach. If the patient is depressed by the colchicum, give Dr. Gregory's powder. Little is to be done locally. If pain is severe in the part, leeches may be applied, and if need be, a bandage for the purpose of limiting motion. When the disease is fully established, these remedies may be still used to mitigate the symptoms, but we must then give the iodide of potash in two or three grain doses twice daily, and continue it for weeks, if it agrees with the patient. No general rule can be laid down. In one case great benefit was found from the use of cod-liver oil locally, and from its administration internally. (Sir B. Brodie, p. 235.)

Excision of the Knee-joint.—Although the operation performed by Mr. Fergusson, and described as follows, was unfortunately fatal, yet it deserves the earnest consideration of the profession, as it has in one instance, at least, been successful. Mr. Fergusson made an H incision in front of the joint, the transverse line running a little below the patella. The extensor tendon was then directed upwards and raised with the patella like a lid. The ligaments were then divided, the soft parts detached from around the bones, one inch and three quarters of the lower end of the femur, as well as the head of the tibia for about three quarters of an inch, were sawn off from before backwards, and the sharp margins of the bones pared down with the forceps. Mr. Fergusson then cut out the patella from its ligamentous attachment, several vessels were secured, and the lines of incision brought together by points of suture. Water-dressing and a roller were subsequently applied round the joint, and the patient removed to bed, where the leg was kept extended with a pillow under the ham. (p. 246.)

Hip-joint disease.—An opening having been made freely with an abscess-lancet, the limb may be wrapped up in a flannel, wrung out of hot water, and this may be continued until the first flow of matter has ceased, a poultice or water-dressing being applied afterwards. The opening should be free, so that the abscess should heal from the bottom; and also that no pressure need be used to cause it to escape. All rough manipulation is to be carefully avoided. The treatment of a sinus which is left after the opening of an abscess may be comprised in a few words. If the orifice be disposed to heal prematurely, it may be prevented by the occasional application of the caustic potash, care being taken that the caustic does not enter the sinus itself; otherwise some simple ointment, or water-dressing, is all that is required. The old practice of probing a sinus yields us no accurate information, nor is it useful: and the same remark applies, but with greater force, to the use of stimulating injections. (Sir B. Brodie, p. 231.)

Several cases of this disease have been successfully treated at St. Thomas's Hospital, by the use of cod-liver oil. It changes the abnormal molecular condition of the blood, so well known to exist in these cases, and so often connected with scrofula and other constitutional disorders. (Dr. C. Kidd, p. 239.)

Hydrarthrosis. M. Velpeau says, in his opinion, there is not more danger in injecting a joint than injecting the tunica vaginalis, and that he has done it with complete success, the smallest possible trocar being employed. Neither does the question of injecting the abdomen in ascites with iodine seem to him one of mere hypothesis. Many French surgeons have related numerous cases followed by complete success, where the practice has been employed, while others consider the conclusions drawn to be somewhat hasty. (p. 244.)

Talipes Varus.—[A new form of apparatus has been invented for extending the leg and foot after the operation for talipes varus, and is at present in use in the London Hospitals; it is described at p.402.]

Tourniquet.—[A new tourniquet, invented by Mr. Skey, is described and figured at p. 406.]

VENEREAL AFFECTIONS.

Gonorrhœa.—Give acetate of potash in half drachm doses, every four hours. Even where the disease has not yielded, it has been repeatedly found, that, after a few doses of copaiba, the cure has been completed, and a relapse prevented. (Mr. J. Hilton, p. 332.)

The severe forms of gonorrhœa which are so exceedingly obstinate, are owing to the inflammation spreading along the entire canal, extending over to the mucous membrane of the bladder itself, nay, even, in some cases to the ureters and kidneys; and pus diffused through the urine is an indication of it. There is no specific remedy for this form, but when pus has once appeared in the urine, the antiphlogistic treatment must be more strictly enforced; and when the inflammatory symptoms have subsided, then, and not until, specific remedies, as balsams and cubebs, may be resorted to. (Mr. W. Colles, p. 334.)

Syphilis.—Surround the patient with an atmosphere of mercurial vapour, in a moist state, by the following method: -Place the patient on a chair, covering him with oil-cloth lined with flannel, then fumigate him with the bisulphuret of mercury, or the grey oxide, or binoxide. The patient may remain in this mercurial atmosphere for ten minutes or half an hour. Let the patient then repose in an arm chair for a short time, and let him drink a cup of warm decoction of guaiacum, sweetened with syrup of sarsaparilla. (Mr. Langston Parker, p. 328.)

Indurated Chancre.—M. Ricord gives the proto-ioduret of mercury, but this does not seem to agree with patients in this country, which may be attributed to climate and difference in diet. He says mercury must be persisted in three or six months after the disease has disappeared to prevent a relapse. He uses fumigations, also, much on the same plan as Mr. Langston Parker, but thinks a spirit-lamp does not give out sufficient heat. (p. 329.)

Phagedenic Chancre.—Iron is the remedy, its effects are almost magical. It is much used by M. Ricord. The following is the best mode of giving it. Potassio-tartrate of iron, one ounce; water six ounces. Mix. Two tablespoonfuls three times a day. (p. 329.)

Tertiary Symptoms.—Give the iodide of potash in *large doses,* which which will benefit when small ones are of no avail. (Mr. W. Acton, p. 330.)

AFFECTIONS OF THE EYE AND EAR.

Corneitis.—In the treatment of diffuse opacity of the cornea from corneitis, by stimulants, Dr. Jacob says, solutions of nitrate of silver, sulphate of copper, sulphate of zinc, or the combination called lapis divinus, will perhaps answer; but he uses a solution of iodide of potassium, ten grains to the ounce of water, or he touches the surface with a camel-hair pencil, previously dipped in water, and brushed two or three times on soap. (p. 454.)

Speculum Auris.—[An improvement on Mr. Wilde's speculum is described by Mr. J. C. Forster, at p. 454.]

MIDWIFERY AND DISEASES OF WOMEN.

Hemorrhage, *Uterine*.—Mr. J. Griffith brings again into notice the great value of oil of turpentine in uterine hemorrhage. He says the most convenient way of giving it is ℥j of turpentine to ℥ss of oil of sweet almonds as a draught, and repeated in five minutes if the symptoms are urgent. (p. 340.)

Lactation.—In cases of child-birth where the appearance of milk is delayed, a decoction is made by boiling well a handful of the white Bofareira (Ricinis Communis of botanists) in six or eight pints of spring water. The breasts are bathed with this decoction for fifteen or twenty minutes. Part of the boiled leaves are then spread thinly over the breasts, and allowed to remain until all moisture has been removed from them by evaporation, and probably, in some measure, by absorption. This operation is repeated at short intervals, until the milk flows upon suction by the child, which it usually does in the course of a few hours. If the milk is required to be produced in the breasts of women who have not given birth to or suckled a child for years, the mode of treatment is as follows:—A similar decoction is made, and it is poured, while yet boiling, into a large vessel, over which the woman sits so as to receive the vapour over her thighs and generative organs, cloths being carefully tucked around her, so as to prevent the escape of the steam. In this position she remains for ten or twelve minutes, or until the decoction cooling a little, she is enabled to bathe the parts with it, which she does for fifteen or twenty minutes more. The breasts are then similarly bathed and gently rubbed with the hands, and the leaves are afterwards applied to them in the manner described. These operations are repeated three times during the first day, on the second the process is repeated as to the breasts three or four times, on the third day the whole process is repeated. A child is now put to the nipple, and, in the majority of instances, it finds an abundant supply of milk. In the event of success not following, the treatment is continued for another day; and if there be still want of success, the case is abandoned. (Dr. J. O. M'William, p. 401.)

Oxytocic.—The administration of the Indian hemp seems markedly and directly to increase parturient action, and Dr. Churchill states, that it possesses powers similar to those of the ergot of rye in arresting hemorrhage, when dependent upon congested states of the impregnated uterus. (Prof. Simpson, p. 401.)

Placenta Prævia.—If the os uteri is not dilated plug the vagina accurately, to produce a coagulum and stay the flooding, to save the child, to prevent the powers of the mother being exhausted by loss of blood, and to favour the dilatation of the os. As soon as the state of the os allows, introduce the hand immediately, separate sufficient of the attachment of the placenta to the uterus to allow the hand to pass, and turn and deliver. We believe that where the cessation of hemorrhage takes place from the separation of the placental attachment, the placenta being still left in situ, as advocated by Professor

Simpson, it is owing to the placenta forming a plug upon the uterine orifices. For the arguments in the defence of the use of the plug and the explanation of its action see p. 338. (Mr. W. Braithwaite.)

Sponge Tents.—An improvement upon Dr. Simpson's method of making sponge tents has been introduced, which consists mainly in not steeping the sponges in a solution of gum; and the advantages of this method are, that "they are made with much greater facility, their being thus compressed into a much smaller compass, and that the expansion of the tents is equally gradual, never requiring the aid of tepid water injections, the moisture of the surrounding parts being quite sufficient." (Mr. C. Coates, p. 388.)

Sterility.—Dr. E. Williams has described a Japanese remedy for sterility. The tree is one of the order of the Ternstromaceæ of Jussieu, with leaves somewhat larger than those of the congou tea, emitting an odour when bruised resembling pulegium and sabina. The mode of preparation is to take a quantity of the leaves, macerate them in as much rice spirit as will just moisten them, for six hours; then express, and give about a teaspoonful every hour, and two or three doses will invariably bring on the menstrual secretion, which can be maintained by a dose or two daily, for any length of time. (p. 406.)

Uterine Neuralgia.—Apply blisters to the hypogastric region, cauterization of the cervix, narcotic injections, with absolute rest and general treatment. (M. Valleix, p. 389.)

Warty Excrescences.—Mr. T. W. Nunn, acting on the principle of Dr. Arnott, of using congelation as an anæsthetic agent, has recently applied little wedged-shaped pieces of ice to the necks of large warty growths depending from the labia minora, till they became blanched and cold; and he then removed a great many of them with a single stroke of the knife, without causing the patient any but very slight pain. (p. 399.)

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D. I. ROEBUCK, PRINTER, 14, BANK STREET, LEEDS.